TIGHT BINDING BOOK

HISTORY OF THE THEORY OF ORE DEPOSITS

WITH A CHAPTER ON THE RISE OF PETROLOGY

BY

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PREFACE

IN this book I have sought first of all to give a fairly complete account of the history of the controversy with reference to the genesis of mineral veins. In so doing, I have treated the subject on general lines and have not attempted to deal with works relating to special minerals, metals or processes. To have treated the subject on special lines would have resulted in an elaborate treatise on ore deposits, one in which the historical considerations, to which I wished to give prominence, would have been lost in a maze of details. I have therefore passed in review only the more outstanding of the contributions of the various authorities who have, from time to time, striven to secure a stable scientific basis for the study of ore genesis.

In looking through the older literature on this subject many years ago, I came to the conclusion that a survey of this literature would serve a useful purpose, not only as showing in an interesting way how slight has been the progress as regards fundamental ideas with reference to the origin of mineral veins during the last century or so, but also as indicating how some of the older authorities had sought to establish a common basis of theory and classification for rocks and ore deposits.

In my paper on *The Genetic Classification of Rocks and Ore Deposits* (Mineralogical Magazine, vol. 17, 1914), I tried to show (1), that a common basis of classification of rocks and ore deposits seemed desirable from the scientific point of view; (2), that such a basis was to be found in the important consideration that the scientific foundation of the study of ore deposits, like that of the study of rocks, was geodynamical; and (3), that the study of ore deposits was

therefore as genuine a part of the science of petrology as was the study of igneous, sedimentary and metamorphic rocks. In that paper I give a scheme of classification which, while it applies consistently to both rocks and ore deposits, satisfies the important requirement of integrity or completeness of grouping, which a division into igneous, sedimentary and metamorphic rocks fails to provide. The reader will find from the account of the work of the early pioneers of petrography I have given in the last chapter, that certain of these pioneers, notably Haüy and Cordier, attached much importance to this principle of integrity or completeness of grouping in their systems of classification, as also did some of their predecessors. This principle of classification has been ignored by nearly all petrologists ever since the commencement of the development of modern microscopical petrography about the middle of the nineteenth century.

Indeed, by looking at mountains through microscopes, to use a well-known sarcastic expression with which field geologists originally greeted the early application of the polarizing microscope to rock studies, petrologists committed the serious error of losing perspective in their views regarding the scope of petrology. They allowed the science of petrology to degenerate for all practical purposes into a study of silicate rock masses under the classification of igneous, sedimentary and metamorphic types, whereas it should have been applied to mineral deposits generally, including ore deposits, as many of the early workers, notably Hutton, Haüy and Cordier clearly meant that it should.

Whether the expediency of treating petrology as the study of silicate rocks will be dominant in the future, as it has been dominant during the last century or so, or whether, in the wider and deeper interests of its completeness and integrity as a science, petrology will ultimately comprise all mineral deposits in its scope, is a question the answer to which the future will provide. Certainly, with its present restricted scope, petrology has no claim to recognition as a definite science. Only by widening its scope to make it

include the study of rocks as a whole can it become a definite and independent science.

If he be desirous of cultivating a wider outlook on this subject, the student could not do better than spend a little time occasionally in a study of its history, and I hope that the following pages will, at least in some measure, make this possible.

THOS. CROOK.

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SUMMARY

FROM early times, fire and water, or more correctly igneous and aqueous agencies, have rivalled one another in significance as speculative factors whereby man has sought to explain the changes in the world around him; and in no branch of study has this rivalry been more persistent than in that concerned with the origin of metalliferous veins.

Early views were doubtless the outcome of man's observations on his immediate surroundings, as, for example, it has been suggested that the story of the Universal Deluge had its origin in the traditional records of extensive floods in the valley of the Euphrates. The influence of this story, with its emphasis on the significance of aqueous agencies, is seen in many later writings.

Greek philosophy at the outset, as represented by Thales, regarded water as the most fundamental substance; but before long, Empedocles on Etna was suggesting from his observations that the earth's interior was in a molten condition, and thereby broadening the basis of speculation. Thus, Aristotle and his followers seem to have invoked both igneous and aqueous agencies to account for the origin of minerals.

After the Greeks, we find no evidence of growth of ideas on this subject until the 16th century, when Bauer (Agricola) for the first time gave a somewhat detailed account of the origin of mineral veins, in which he put the emphasis on aqueous agency. The water represented by his circulating juices (*succi*) was water of meteoric origin; and he allowed no scope at all for the fiery exhalations of the Peripatetic school.

In the following (17th) century, however, we find Descartes assigning an igneous origin to metalliferous veins,

the formation of which he attributed to vapours and exhalations arising in the earth's interior. Hard on his heels came John Woodward to champion the story of the Universal Deluge and maintain the claims of meteoric waters in explaining the genesis of mineral veins.

These rival claims jostled each other freely in the active speculation that took place during the revival of interest in this subject during the 18th century, and culminated in the vigorous controversy between the Plutonists (Huttonians) and Neptunists (Wernerians) at the end of that century and the beginning of the 19th. Although that dispute was concerned chiefly with the origin of igneous rocks, it involved also the problem of the origin of mineral veins.

A lull followed this period of active controversy, and in the second quarter of the 19th century a sort of compromise was effected. The notion developed that, whereas igneous action was important, the co-operation of aqueous agency was necessary for the formation of both igneous rocks and metalliferous veins. Elie de Beaumont and Von Cotta secured predominance for this modification of the igneous theory, although Fournet still favoured the action of dry magmas for the genesis of metalliferous veins as Hutton had done previously.

In the second half of the 19th century, opinions among different authorities were well divided. Belt asserted the claims of dry igneous magmas, while Daubrée, Bischof, Sandberger, Phillips, Sterry Hunt, Emmons, Van Hise and others maintained those of meteoric waters. The compromise school was strongly represented by Von Cotta, Stelzner, Pošepný, de Launay and Vogt, and this school maintained its predominance, with strong emphasis on the igneous factor as an essential feature.

During the present century, de Launay and Vogt in Europe, and Kemp and Lindgren in the United States, have been chiefly responsible for maintaining the prestige of the igneous theory, on to which there has been a strong tendency to graft the notion of the juvenile origin of igneous water as

advocated by Suess and the further notion of the baryspheric origin of the metals, as advocated by de Launay and Pošepný.

The chief development in recent years has been the revival by Spurr of the dry-magma theory formerly advocated by Hutton, Fournet and Belt, a revival in which he seems to have obtained a considerable measure of support. We see, therefore, that in one way or another, the position is held by the igneous theory of origin for metalliferous veins, with strong emphasis on the juvenility of the water and metals involved in their formation.

That such should be the case is rather remarkable, in view of the shallow limitation in depth and sporadic lateral distribution of metalliferous veins. These facts, and the mineral complexity of many vein deposits, together with the insignificance of basic magmas as carriers of lode metals, and the probability of the shallow origin of granite intrusions in core folds, cannot be said to be in favour of the juvenility of either the water or the metals involved in the genesis of mineral veins.

Further, the association of metalliferous veins and igneous intrusions is far too accidental, variable and irregular to be explained on the assumption that the metals are juvenile in these intrusions. The sporadic lateral distribution of metalliferous deposits in what have been termed "metallogenic provinces" is not best explained by assuming a baryspheric origin for the metals. So far as igneous action may be involved in it, it is better explained as a resurgence effect accompanying tangential compression and igneous intrusion, and affecting metalliferous matter already existing in the covering rocks of the earth's outer crust.

We know, moreover, that in continental areas, the earth's outer crust, in which metalliferous veins have been formed, has been from the early days of the earth's history the seat of sedimentary and metamorphic action, and that igneous agencies have played a comparatively subordinate part. In keeping with this important fact, are the further

facts, (1) that the mineral output representing the commoner oxides making up some 99½ per cent. of the earth's outer crust is almost entirely due to concentration by the sedimentary and metamorphic agencies just referred to; and (2) that 85 or 90 per cent. of the value of the world's total annual output of minerals at the present time is from concentrations in the outer crust that can safely be attributed to these same sedimentary and metamorphic agencies, in which meteoric water has played an overwhelmingly predominant part.

We may therefore reasonably infer that the modern igneous theory of metalliferous veins is not in harmony with the main facts to be accounted for, and that future investigation is likely to diminish the prestige of this theory. Indeed, a little hard thinking on the facts already known will go far in that direction, and do something to secure for exogene agencies the recognition due to them as the chief agencies by which metalliferous and other minerals have been concentrated in the outer crust of the earth.

From a consideration of the rise of petrology, it is shown that this subject had a wider and more rational scientific basis in the minds of some of its pioneers; and it is suggested that the scope of modern petrology needs to be widened to make it adequate for scientific purposes.

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CHAPTER I

INTRODUCTION

THE problem of the origin of ore deposits, and more especially that of the origin of vein deposits, has proved an attractive subject of speculation throughout the history of modern science. Even before the birth of the science of dynamical geology, *i.e.*, before the end of the 18th century, active speculation had taken place on this subject, not only among those who realized its economic importance in connection with mineral production, but also among many who were interested in it as a scientific or natural history problem. In its bearing on mineral resources, the importance of the problem of ore genesis seems obvious. Its bearing on geodynamics is perhaps less obvious; but, as was fully realized by the early pioneers of the science of geology, its adequate consideration is inevitably bound up with dynamical geology, and cannot safely be avoided by anyone who wishes to understand the structure of the earth and the processes by which that structure has been developed.

A study of the history of the theory of ore deposits shows that modern authors have not added very much of fundamental importance to the general conceptions set forth by early workers. Such a study furnishes excellent examples of the proverb that there is nothing new under the sun, providing as it does many instances of the publication of suggestions and hypotheses consisting of mere re-statements of suggestions and hypotheses clearly made or elaborated by previous workers. Indeed, a little consideration suffices to show that there is fundamentally very little choice of alternatives in theorizing broadly on the origin of ore deposits, and when the views held at various times are reduced to

simple terms, there is seen to be not much variety among them.

The mineral matter of veins has either been derived from the country rock traversed by the veins, or it has been deposited by solutions or melts originating elsewhere. If elsewhere, then the solutions have been either descending (vadose), or ascending (juvenile or resurgent), or both. These few simple general conditions box the compass of possibilities, and it is around them that controversy has been waged, usually with a strong preponderance of opinion in favour of some simple view of the problem.

Although, however, the speculations of modern workers on the origin of vein deposits may not be seriously different from those of older workers, it is far otherwise as regards their knowledge of the facts of ore deposition. The results of modern scientific inquiry have indeed placed at the command of the present day worker a vast amount of knowledge concerning this subject. In this, as in other branches of inductive science, the ponderous work of fact collecting has grown apace, to such an extent that there is at the present time an almost embarrassing wealth of data available, a state of things which tends to act as a depressant rather than as a stimulant to anyone who wishes to survey the problem as a whole for the purpose of testing the validity of the dominant theories.

With all this wealth of data, one might be excused for thinking that it should be possible to find an adequate foundation for a theory of the origin of metalliferous veins, and that any theory now dominant, having drawn its strengthening elements from the ever-increasing data of observation, would be quite valid, and secure against any possibility of being overthrown. That such is not the case, however, must be quite patent to anyone who has made a thorough survey of the history of controversy on this question, in relation to the scientific principles involved.

It should of course be admitted that the pertinent facts required to clinch any theory of the genesis of metalliferous

veins have hitherto not been brought to the test of observation and experiment. That they never will be brought to this test is far from probable; but until the clinching observations and experiments have been made, it is more harmful than beneficial to pretend, as many do, that the particular theory they entertain is the only one that is tolerable. Unfortunately, in this as in other branches of thought, man is impatient with the slow rate at which truth is revealed. Unsatisfied to hold his judgment in suspense in the presence of conflicting data, and unwilling to seek very far or very laboriously for the bases of his belief, he prefers a simple and direct hypothesis, the entertainment of which frees him from the irksomeness involved in a cautious application of the principles of science.

This desire, possessed by so many, for a simple and direct hypothesis of wide application, is amply illustrated by the outstanding features in the history of the theory of ore deposits. We see it in the neptunistic theory of Werner, that ore deposits owe their formation to deposition from surface waters, uncomplicated by any further action either by leaching or by thermodynamic processes. We see it also in the alternative plutonistic theory of Hutton, that metalliferous veins have originated as igneous injections. Both these extreme views had formerly a large measure of success, and were widely entertained to the exclusion of other views.

We see again the same desire seeking expression in the work of Sandberger, whose statement in 1873 of the rather crude lateral-secretion hypothesis in its extreme form, promptly attained a wide measure of success, due no doubt to the fact that it constituted a healthy reaction against the extremist views of the ascensionist school, while at the same time it offered a simple (alas! too simple) and readily understood explanation of the formation of metalliferous veins.

Following this came another reaction, this time engineered by Pošepný in 1893, who affirmed the hypothesis that "primary" ore deposits were due to the action of metalliferous solutions of baryspheric origin, a view of things still

entertained by de Launay and some other modern authorities. It is, indeed, puzzling to understand how anyone can find adequate geological grounds for this hypothesis, and difficult to realize how it could win its way, except on the assumption made above that there exists in these matters a strong preference among the majority of people for a simple and direct view of things. Sandberger's lateral secretion theory had been discredited in its extreme form, and students of ore deposits set little store on geodynamical factors in ore genesis. Circumstances thus favoured reaction, and in spite of his somewhat irrational emphasis on the barysphere as the source of the metals in metalliferous veins, Pošepný carried the day in favour of the igneous theory of ore genesis, and since that time this theory has dominated the field.

The form in which the igneous theory ultimately became established, however, was not that proposed by Pošepný, but that previously elaborated by Elie de Beaumont, Von Cotta and other early workers, and advocated more recently by Kemp, Lindgren and others. According to this theory, which again in its restricted form represents a simple view of the problem, sulphidic mineral veins, and indeed those of other types, have in large measure been derived from vapours and solutions emitted from igneous intrusions, and have been deposited either directly or by replacement in the fissures or substances of the superincumbent rocks. In this form, qualified in recent years by the notion of zonary deposition controlled by varying conditions of temperature and pressure, the igneous theory has had a long and very successful run.

Still more recently the changes have tended to come full circle as a result of Spurr's advocacy of the "ore-magma" or "vein-dike" hypothesis, a view of things which is essentially similar to that entertained by the plutonists during the half century or so from James Hutton onward, and one which found strong advocates in Fournet and some other workers who were firm believers in dry magmas. Here again we see working the strong desire for a clear-cut and

simple conception, the entertainment of which will relieve the mind from those tedious operations of thought and judgment involved in the consideration of the merits of any particular case.

Experience shows quite clearly, however, that no simple hypothesis will explain the origin of so-called "primary" vein deposits in all cases. The more fully the facts are studied, the clearer it becomes that a cautious application of the principles of dynamical geology is inevitably involved in any satisfactory effort that one may make to understand the origin of metalliferous veins and other mineral deposits. This view of things becomes quite convincing when one realizes that mineral deposits (including ore deposits) are rocks, the genesis of which is essentially bound up with the study of geodynamics. Throughout the history of controversy on this subject there has been scant recognition of the important fact that the proper scientific basis for the study of ore deposits is geodynamical. Chemistry, physics and physical chemistry have been tried and are still being tried as scientific bases on which to build an interpretation of the facts of ore genesis, as also those of petrology, of which ore genesis forms a part. Any amount of success that may be attained in these directions, however, will still render necessary the scientific development of geodynamics as a study of processes in relation to rock genesis as a whole, including ore genesis. More than this, it may be claimed that the study of the physical chemistry of rocks can only become scientifically effective as a branch of geodynamics, which is the main trunk of the science of geology.

The need for emphasizing this important consideration becomes the more apparent when one realizes how very backward in its scientific development geodynamics really is, and how serious is the neglect of many branches of investigation that this backwardness of development involves. Petrologists have in the past been too much inclined to regard petrology as the science of igneous rocks, and many of them have been so narrow in their outlook as to fancy that a science could be

made out of the study and description of rock specimens, preferably specimens of igneous rocks. Together with this narrow outlook of petrographers as regards their science, and indeed correlated with it, is the corresponding neglect of those aspects of geodynamics the necessary development of which is inevitably bound up with a fuller development of petrology. It is from this development of geodynamics, or the study of rock genesis, together with an ampler study of rocks themselves as a whole, that most help is likely to be derived in settling many of the large and vexed problems that still await solution in ore genesis. In attacking these problems, geologists have been too prone in the past, as indeed they are to-day, to throw their responsibilities on to the shoulders of votaries of other sciences, who to their credit have tried in vain to shoulder them. It is, however, not necessary to depreciate the valuable assistance that other sciences have given, or are likely to continue to give to petrology and ore genesis, in urging the need that exists for students of geology to continue the scientific development of geodynamics; for without this development it is hardly possible that many modern problems, the solution of which has been too long delayed, will ever be solved, however much assistance other sciences may give.

CHAPTER II

THE VIEWS OF THE ANCIENTS

RECORDS of the study and investigation of mineral veins and their genesis extend back through several centuries. The ancient schools of learning, including those of the Greeks and the Romans, and, in the Middle Ages, the Arabians, showed little or no interest in this subject, for the simple reason that a knowledge of minerals and geological processes had not sufficiently emerged in their times to make its study possible. Among the philosophers of these ancient schools, the nature of the earth and earth changes were merely matters of speculation. They knew very little about minerals, and almost nothing at all about rocks; and they were quite contented to have no basis of fact, or at most a very slender basis, for their speculations.

Various old writers postulated a universal deluge to account for the earth's condition, a view of things which, on account of its intimate association with Hebrew and Christian traditions, has persisted down to modern times. In later Hebrew writings, we get a suggestion of igneous agency in connection with the mention of metals and minerals, as in the book of Job, where it is stated: "Surely there is a vein for silver and a place for gold. . . . As for the earth. . . . Iron is taken out of it, and brass is molten out of the stones. . . . The stones of it are the places of sapphire, and it hath dust of gold . . . and under it is turned up as it were by fire."

Among the Greek philosophers, Thales (about 640 B.C.), the Father of Greek philosophy, regarded water as the most fundamentally important of all substances. He taught

that water or moisture was the primordial element from which all things had been evolved. In sharp contradistinction to this notion was that of Zeno (about 340 to 270 B.C.), that fire was the primordial element from which all things had arisen.

Less irrational than these was the teaching of Empedocles (492-432 B.C.), who regarded the universe as being built up of four elements, namely, fire, water, earth and air, through the varied interaction of which, world changes were due. Empedocles, moreover, sought to some extent to find a basis of actual observation to support his speculations. He held the view that the earth's interior was in a molten condition, and based this view on the fact that, at Mount Etna, such molten material was emitted by the volcano. Aristotle (384-322 B.C.) also believed in the four elements of Empedocles as explaining the nature of matter. According to him, these elements suffered various changes in the earth's interior, as a result of which exhalations were given off. Some of these exhalations were fiery and produced stones, whereas others were moist and gave rise to the metals. Theophrastus (372-287 B.C.), Aristotle's pupil and successor, wrote a book *Of Stones,* which is the oldest treatise extant on minerals. It is noteworthy that this book is essentially an economic mineralogy, dealing as it does with minerals in a practical way and giving an account of their uses. Theophrastus grouped his minerals as *Metals, Stones* and *Earths,* and the mineral species he described were 16 in number.

It is clear from the foregoing account that the Greeks knew little of geophysical and geochemical processes. Their knowledge of minerals was indeed very scanty, and altogether insufficient to enable them to evolve any satisfactory explanation of the genesis of rocks and minerals.

This state of comparative ignorance concerning rocks and minerals continued among the Romans, who appear to have added nothing of note on this subject to the knowledge of the Greeks, either in the way of facts or speculations.

Not until the Middle Ages do we find among existing records any evidence of progress in the study of minerals and that is very slight. Avicenna (980-1037), the Arabian translator of Aristotle, grouped minerals as *Stones, Sulphur minerals, Metals* and *Salts*. This represents an advance on Theophrastus, who appears to have regarded metallic sulphides as metals on account of their lustre. Avicenna's grouping was roughly similar to that adopted by Agricola some centuries later (see below), and indeed it persisted with slight variation until towards the end of the 18th century.

CHAPTER III

SIXTEENTH CENTURY

AGRICOLA (G. BAUER)

BETWEEN the Middle Ages and the 18th century, the author who merits chief attention on account of his contribution to the study of minerals, their properties and their genesis, was George Bauer (1494-1555), better known by his pen-name Georgius Agricola, who made a very substantial advance on the writings of previous authors, although he appears to have inherited from the Schoolmen of the Middle Ages a deep veneration for the teachings of Aristotle and the Peripatetics generally, and his broad classification of " homogeneous " minerals in *De Natura Fossilium* (1546; 2nd edition 1558) was not widely different from that referred to above as adopted by Avicenna, though it was much more elaborate. Agricola, however, made a broad distinction between rocks and minerals, which represents an important advance on the ideas of older workers. In his classification, Agricola divided mineral substances into two main groups, namely, (1) *Homogeneous minerals,* and (2) *Heterogeneous mixtures of minerals.* The second of these groups corresponds with what we call rocks. Homogeneous minerals he divided into *Earths, Salts, Gemstones, Metals* and *Other Minerals.* He remarked that, whereas in homogeneous minerals the constituents could be separated by means of heat, in heterogeneous mixtures they could sometimes be separated by water and often by hand. The division of homogeneous minerals into groups just referred to, is a slightly simplified rendering of Agricola's actual arrange-

ment, simplified for the purpose of showing its rough resemblance to that of Avicenna on one hand, and the schemes of later writers, notably that of Werner, on the other.

Metalliferous veins were regarded by Agricola as " canales " or openings that had been infilled by matter deposited from underground waters. He dealt with this and kindred matters relating to the processes involved in ore-genesis in his *De Ortu et Causis Subterraneorum* (1546; 2nd edition 1558). He attributed the formation of all these " canales " (fissures as well as other kinds of cavities) to " erosion " by underground waters. If by " erosion " he meant to include the solution effects of circulating underground waters, he was to this extent correct; but he failed to realize that most fissures, as was indicated later by Descartes (1644) and still later confirmed by Von Oppel (1749) and others, are due to dynamic adjustments in the rock masses of the earth's crust.

Agricola regarded underground waters as derived in part directly from surface waters (rain, river and sea water) which had seeped into the earth; and in part indirectly from such surface waters as had penetrated sufficiently far into the earth's crust to become heated and vaporized, and thus driven outward as steam and condensed in the shallower parts of the crust. He attributed the production of heat in these deeper portions of the earth's crust to the combustion of coal beds, bitumen, etc.

Although Agricola distinguished or classified different kinds of ore deposits by form, according as they were ordinary veins (" vena profunda ") or other kinds, such as beds, stocks, stringers, etc., his classification of mineral substances was in a way genetic, since he regarded the different groups as arising from the action of different kinds of " succus " or juice, coupled with the effects of heat and cold.

As to the nature of the circulating juices (" succi ") or solutions from which vein minerals were deposited, the views set forth by Agricola were in the main very fantastic, and

were elaborated with all the love of complication characteristic of the work of the Schoolmen. It should be mentioned, however, that he claimed superiority for his view of things on the ground that it was based on actual observation rather than on mere reasoning and speculation. He made many accurate observations on such phenomena as the decomposition of rocks by rain water, the alteration of copper by moisture, the decomposition of metallic sulphides such as copper- and iron-pyrites by moisture, and the ordinary process of petrifaction by the deposition of mineral matter from aqueous solutions (" succus lapidescens ").

Agricola's writings, indeed, represent an important scientific advance on those of his predecessors, and served as standard works for a long time. The works of later writers on minerals and ore deposits during the 18th century clearly owed much to the influence of Agricola, who stands out prominently in the history of this subject as one of the most original contributors to the study of ore genesis.

Readers who wish to pursue the study of Agricola's views on this and kindred questions should refer to an excellent translation of his *De Re Metallica* by H. C. and L. H. Hoover, published in 1912 by The Mining Magazine, London.

CHAPTER IV

SEVENTEENTH CENTURY

DESCARTES, STENO AND WOODWARD

AFTER Agricola, the actual study of ore deposits, for the purpose of ascertaining their genesis, appears to have fallen into abeyance until the eighteenth century, so far as can be judged from existing records, or the absence of such. It is, however, of some importance to note the views of René Descartes and Nikolaus Steno, both of whom, during the 17th century, formulated very definite views as to the nature and cause of the earth movements which are chiefly responsible for large fractures or fissures in the rocks of the earth's crust.

In his *Principia Philosophæ,* published in 1644, Descartes, among the other splendid guesses he made at scientific truth, regarded the earth as a small cooled star, only the crust of which had been chilled, while the interior still remained very hot. To this condition of things he attributed the dislocations that had taken place in the outer envelope, as well as the resurgence of waters of infiltration towards the surface, and the deposition of mineral veins. He held the view that, below the stony outer crust of the earth, there was a shell of very heavy matter from which came the metallic minerals found in veins traversing the outer crust. He inferred that these vein minerals represented material that had been driven outward as exhalations towards the surface by the earth's internal heat, and deposited in the fissures of the cooled outer crust. This theory of the existence in the earth of a deep-lying shell of metalliferous matter, from which the metalliferous vein

minerals of the crust are derived either as injections or through the medium of solutions and exhalations, finds advocates among some modern authorities.

The following passages from Part IV (The Earth), of Descartes' *Principia Philosophæ,* are quoted from a French translation:—

" Outre les vapeurs qui s'élèvent des eaux, il sort aussi de la terre intérieure grande quantité d'esprits pénétrants et corrosifs, et plusieurs exhalaisons grasses ou huileuses, et même de l'argent vif, lequel montant en forme de vapeurs, amene avec soi des parties des autres métaux; et selon les diverses façons que ces choses se mêlent ensemble, elles composent divers minéraux. . . . Ainsi les vapeurs de l'argent vif qui montent par les petites fentes et les plus larges pores de la terre, amenent aussi avec soi des parties d'or, d'argent, de plomb, ou de quelqu'autre métail lesquelles y demeurent par après, bien que souvent l'argent vif ne s'y arrête pas, à cause qu'étant fort fluide il passe outre, ou bien redescend. Mais il arrive aussi quelquefois qu'il s'y arrête, a sçavoir lorsqu'il rencontre plusieurs exhalaisons, dont les parties fort déliées envelopent les siennes, et par ce moyen le changent en vermillon. Au reste, ce n'est pas le seul argent vif qui peut amener avec soi les métaux de la terre intérieure en l'extérieure, les esprits et les exhalaisons sont aussi le semblable au regard de quelques-uns, comme du cuivre, du fer, et l'antimoine. . . . Il faut aussi remarquer, que c'est ordinairement par le pied des montagnes que montent ces métaux."

In his *De solido intra solidum naturaliter contento* (1669) Steno set forth sound views on dynamical geology, and showed how the stratified rocks of the earth's crust had been dislocated by earth movement subsequent to deposition. He further showed how the folds and fractures arising from elevation and subsidence in the crust of the earth, together with the effects of denudation, explained the origin of mountains, valleys and other features of the earth's surface. Steno thus confirmed Descartes' views as to the origin of crustal fractures and fissures. The view that the formation of such fractures preceded the infilling of metalliferous vein matter in what are known as fissure veins, was, however, much debated among authorities on ore deposits during the 18th century, before it came to be ultimately established and recognized as correct.

In *An Essay towards a Natural History of the Earth and Terrestrial Bodies, especially Minerals* (1665), John Woodward, F.R.S., gave an account of the "Universal Deluge," and the effects that it had upon the earth. In this book, unlike Descartes, he would allow no scope to the action of the earth's internal heat in connection with the formation of mineral veins. On the contrary, he put forward a fairly definite statement of the lateral secretion hypothesis, in which he claimed

> "that the metallick and mineral matter, which is now found in the perpendicular intervalls of the strata, was all of it originally, and at the time of the Deluge, lodged in the bodies of those strata . . . that it was educed thence and transmitted into these intervalls since that time, the intervalls themselves not existing till the strata were formed . . . that there do still happen, transitions and removes of it, in the solid strata, from one part of the same stratum to another part of it, occasioned by the motion of the vapour towards the perpendicular intervalls of these" (2nd Edition, 1702).

By "perpendicular intervalls" Woodward clearly meant fissures, and he used this word elsewhere for these divisional planes in strata. By his remark that they did not exist until the strata were formed, he presumably meant to deny any connection between them and the assumed deeper-seated fissures through which Descartes had imagined that vapours and exhalations emerged from the earth's interior. Barring the Deluge, this shot by Woodward was not at all a bad one, and it well deserves a place in the record of early speculation on the origin of mineral veins.

CHAPTER V

EIGHTEENTH CENTURY (FIRST THREE QUARTERS)

SWEDISH, GERMAN AND ENGLISH WRITERS

DURING the eighteenth century there was a revival of interest in the study of ore deposits, more especially among the mining engineers and mineralogists of the famous Freiberg mining district in Saxony, although the outcome of the numerous discussions and publications on ore-genesis by these workers in Saxony during that time can scarcely be regarded as showing much originality.

In dealing with this period, mention should be made of the activity of the Swedish mineralogists, as their writings appear to have had much influence in shaping the views of Werner, the far-famed professor of the mining school at Freiberg, whose personality as a teacher made him such a weighty influence in Europe during the last quarter of the eighteenth century and the first quarter of the nineteenth.

As early as 1722, Swedenborg had attributed a sedimentary origin to Swedish trap rocks; and in 1756, Linnæus also assigned an aqueous origin to the Kinnekulle trap rocks of Sweden. This view, which was adopted by Werner, became the subject of a long and heated controversy between the Neptunists, as the followers of Werner were called, and the Plutonists, who adopted Hutton's view (see below) that such trap rocks were of igneous origin. This controversy had an important bearing on the theory of the origin of metalliferous veins, as it was concerned with the rôle of water in igneous phenomena, the consideration of which was

to prove such an important item in the further development of the theory of ore genesis during the nineteenth century.

The Swedes, as represented by Linnæus, Wallerius, Cronstedt, Bergman and Tilas, also did highly valuable work during the eighteenth century in connection with the study of rocks and minerals from the standpoint of description and classification, work which, through its adoption in large part by Werner, who incorporated it in his lectures at Freiberg, had important effects in pushing forward the development of mineralogy, petrography and stratigraphy, although it had little or no direct bearing on the study of ore deposits.

As regards the work on ore genesis published by German writers during the revival referred to above, most of it was highly speculative. Various theories were advanced by different authors, and some of these anticipated in a remarkably definite manner the theories that have had such a wide vogue at various later times.

As already mentioned in connection with Descartes and Steno, Von Oppel, in his *Anleitung zur Markscheidekunst* (1749) advocated the view that the formation of metalliferous veins had been preceded by fissuring which had taken place prior to the circulation of the ore-bearing solutions from which the veinstuff had been deposited; but some time had yet to elapse before the nature and formation of different kinds of vein fissures were understood.

The following four paragraphs, down to and including Lehmann, are based on information given by Werner in his *Neue Theorie*:—

In his *Pyritologia* (1725), Henkel explained the formation of metalliferous lodes as due to deposition from vapours arising from fermentation in the rock substance of the earth.

C. F. Zimmerman, of the Mining School of Upper Saxony, in 1746, anticipated the lateral-secretion hypothesis by declaring that mineral veins owed their formation to the transformation ot the substance of the enclosing rock.

A later and more definite statement of this view was that made by C. T. Delius in *Abhandlung vom Ursprung der Gebirge und der darin befindlichen Erzadern* (1770), in which

he regarded veins as fissures that have been formed by the drying and subsequent contraction of the rocks. He attributed the infilling of the fissures to the deposition of mineral matter from rain water that had traversed the country rock, and that had taken up mineral matter in solution or suspension during the seepage.

J. G. Lehmann, a teacher of mining and mineralogy in Berlin, notable among early geologists for his pioneer stratigraphical work on the Permian formation of Thuringia, advocated views as to the origin of metalliferous veins not unlike those already referred to as set forth by Descartes, and those advanced by Elie de Beaumont and others during the 19th century. In his account *Von den Metalmuttern und der Erzeugung der Metalle* (1753), he stated that vein minerals had been deposited in fissures by vapours and exhalations emanating from the earth's interior. According to him, lodes were comparable with the branches of a tree, and had their roots deep in the core of the earth.

W. Pryce, in his *Mineralogia Cornubiensis* (1778), explained the formation of veins by a process of lateral secretion, as follows:—"We may reasonably infer that water, in its passage through the earth to the principal fissures, imbibes, together with the natural salts and acids, the mineral and metallic particles with which the strata are impregnated." The metallic and mineral matter thus dissolved are precipitated in the fissures to "form different ores more or less homogeneous, and more or less rich according to the different mixtures which the acid had held dissolved, and the nidus in which it is deposited."

These and other speculations prevalent during the eighteenth century show clearly that the various possibilities as regards the modes of origin of mineral veins had been freely considered in a general way before the time of Werner and Hutton; and, however much we may be inclined to regard these early speculations as lacking in scientific foundation, we may reasonably doubt whether, in this respect, they were, after all, very far inferior to the dominant speculations entertained on this subject at the present day.

CHAPTER VI

EIGHTEENTH CENTURY (FOURTH QUARTER)

J. HUTTON AND A. G. WERNER

JAMES HUTTON was a Scotsman who, although trained for medicine, gave the best energies of his life, as a man of independent means, and an amateur geologist, to the investigation of the structure of the earth, and more particularly to the study of geological processes and rock genesis. Little prone to publish his views, which were perhaps too original to excite much attention among his contemporaries, there can be little doubt that his statement of his views was ultimately all the more effective for the reserve he displayed during the long period of careful observation and study involved in their formulation.

Hutton was born in 1726; but it was not until 1788 that his *Theory of the Earth* was published as a paper in the Transactions of the Royal Society of Edinburgh. In this paper he defined for the first time the real position as to the origin of plutonic and metamorphic rocks, and credit is due to him chiefly for the establishment of the fundamental truths concerning petrogenesis and dynamical geology.

In addition to this, however, it is worthy of note by those interested in the theory of ore deposits, that, in the survey he made of the field of geodynamics in his *Theory of the Earth,* Hutton attached importance to sulphidic rocks and vein deposits. In fact, he exaggerated their importance, as, excusably enough in those early days, he exaggerated that of thermal metamorphism; and if later workers found it convenient to divorce the study of ore deposits from that of rocks, with the result that the scientific development of

these studies was cramped and inhibited, it is clear that in doing this they were departing from the practice adopted by Hutton, who had sound views as to the desirability of giving a wide scope to the study of rocks and geodynamics.

Abraham Gottlob Werner (1749-1817) was much like Hutton in his reluctance to commit his views to writing, although, whereas the influence on the minds of his contemporaries of the amateur Hutton was very weak, that of the professional Werner was very strong. Werner was the popular Professor of his time, and students flocked to his lectures from all parts of Europe, returning to their native haunts as missionaries zealous to proclaim and defend his views. Hence, although young as compared with Hutton, he was very famous and influential, and his *Neue Theorie von der Entstehung der Gänge* (1791) was widely regarded as the correct account of the origin of vein deposits.

In this work, Werner attributed the formation of veins to the infilling of cracks by solutions percolating into them from above. He connected this deposition with the process of rock formation, suggesting that the vein matter was deposited in fissures of pre-existing rocks by the waters from which the overlying sediments were precipitated. He regarded what we call igneous and metamorphic rocks, other than volcanic ejectamenta, as having been deposited from surface waters in the same way as sedimentary rocks. According to him, granite, gneiss and schist were primitive rocks (*Urgebirge*) deposited from aqueous solutions at an early stage in the history of the earth. Where basaltic sills occurred interbedded with sandstones and shales, he claimed that there was a gradual transition from the basalt to the associated sandstone and shale, and that the basalt, like the sandstone and shale, was formed by aqueous deposition.

Hutton showed clearly, however, that the facts concerning the nature of granite, basalt and other igneous rocks, and their relation to the sediments which they had invaded and metamorphosed, could only be adequately explained on the assumption that they had been formed as molten magmas

at deeper levels in the earth's crust. It required a good fight, however, to establish this plutonistic view, for it met with strong opposition from the numerous Neptunists, who persisted in supporting the views advocated by Werner.

In this contest, Plutonism was ably advocated by Hutton's friend, John Playfair, whose clear exposition in his *Illustrations of the Huttonian Theory* (1802) had much to do with the success of that theory. As a result of this propaganda by Playfair, and the experimental work of Sir James Hall, the actual facts as to the nature of igneous rocks and geodynamical processes became better known, and Neptunism in its extreme form gradually lost ground so far as igneous and metamorphic rocks were concerned. It cannot be claimed, however, that this conquest of the igneous theory extended to metalliferous vein deposits, although the Plutonists continued to regard these as due to the direct action of igneous magmas.

Werner, as we have seen, held that granite, basalt and vein deposits all owed their formation to deposition from aqueous solutions. Igneous action in any form was excluded by him from processes of ore deposition.

Hutton went to the other extreme, and thought it just as erroneous to attribute the formation of metalliferous veins to aqueous deposition as it was to attribute the formation of igneous rocks to that cause. According to him there were two kinds of mineral substances in the earth, namely, (1) siliceous, and (2) sulphurous. He contended that siliceous matter was insoluble in water and that silicates and silica must therefore have been formed by fusion. In proof of this, he pointed to the evidence provided by the intrusion of igneous rocks, and the thermal alteration effects these intrusions had produced on the rocks they had invaded. He applied the same reasoning to sulphurous substances. Sulphur, he argued, was fusible by heat, but not soluble in water. Metallic sulphides were of indefinite variety; but as they were not soluble in water, they could not have been deposited by that solvent.

Pyrites, blende, galena, barytes, fluor, calcite and quartz, explained Hutton, were found mixed together in veins in such a way that they might be said to contain each other. They frequently show their crystalline shape. This, and the fact that they were not soluble, whereas they could be melted by heat and transported as molten products, he regarded as a clear indication that the sulphidic minerals of metalliferous veins had been injected as molten material, and not deposited from solutions as claimed by Werner.

Hence, according to Hutton, the formation of metalliferous veins merely required for its explanation the assumption of thermodynamical changes in the rocks concerned; or to use his own words:—

> "No further conditions are required than the supposition of a sufficient intensity of subterraneous fire or heat, and a sufficient degree of compression upon those bodies which are to be subjected to that violent heat without calcination or change" (*Op. cit.*, p. 231).

Hutton thus applied to the explanation of the origin of sulphidic vein deposits exactly the same reasoning as that by which he explained the origin of igneous rocks. In both cases they were due, according to him, to crystallization from comparatively dry melts in which water played no important part. In other words, he was an ore-magma extremist; and it is indeed quite easy to see that he had almost no alternative to this view, believing as he did that practically all compacted rocks, sedimentary as well as igneous, owed their condition to the action of thermodynamic agencies. Indeed, the admission by the Plutonists of the importance of water as an agent in connection with the origin of igneous rocks and vein deposits would have substantially weakened their position, and would have hampered them in their controversy with the Neptunists.

It should be pointed out, therefore, that the Wernerians recognized what the Huttonians failed to recognize, namely, the significance of the action of aqueous solutions as a factor in petrogenesis; and if they exaggerated that significance, and also erred in regarding solutions at low temperature as

solely responsible for the origin of igneous and metamorphic rocks, it should be said on their behalf, that the development of opinion during the 19th century was in the nature of a compromise between the plutonistic and neptunistic views, inasmuch as, while it fully recognized the significance of molten magmas, it insisted more and more on the importance of water as an agent in the formation of igneous and metamorphic rocks, as well as of vein deposits.

CHAPTER VII

NINETEENTH CENTURY (FIRST QUARTER)

HEIM, CORDIER, DOLOMIEU, BREISLAK, SCHMIDT, BOUÉ AND SCROPE

THE first quarter of the nineteenth century was a period of stagnation as regards the theory of ore deposits. It took a long time for the Plutonist theory as regards igneous rocks to win recognition on the Continent. It could hardly be expected to secure anything but tardy recognition in Germany; but in fact its recognition in France also was a thing of slow development; and in this connection it should be remembered that the leaders of geological opinion in Paris had received their instruction from Werner.

Playfair's *Illustrations of the Huttonian Theory,* which was published in the year 1802, was not translated into French until 1815. In it, Playfair reiterated without amplification Hutton's theory of vein deposits, remarking that "the materials which fill the mineral veins were melted by heat and forcibly injected into clefts and fissures of the strata." Hutton's arguments for this view of things were, as already indicated, much the same as those he applied in favour of the molten origin of ordinary igneous rocks. Whereas, however, his observations on igneous rocks and their relation to the rocks they had invaded were very extensive, he had, as compared with Werner, very little experience as an observer of metalliferous vein-rocks, and on that account his views concerning the origin of the latter carried less authority, although it was natural enough that the success of his plutonic theory of igneous intrusions should

strengthen his claims as regards the molten origin of metalliferous veins.

One after another the prominent authorities who had studied under Werner, and who had started their careers imbued by his influence, notably Von Buch, Von Humboldt, and D'Aubuisson, abandoned the Neptunistic conception as regards igneous rocks, finding it inconsistent with the results of their field observations. Those workers who knew most about metalliferous veins, however, had no sympathy with the injection theory as applied to veins. They regarded it as incapable of explaining some of the most important facts concerning these veins, more especially the fact of crustification and the thinning out of veins in depth. Moreover, there was no such clear evidence of intrusive relationship between ordinary veins and enclosing rock as there was in the case of granites and other igneous rocks.

It is hardly surprising, therefore, that the injection theory of Hutton and his followers did not prosper in its application to metalliferous veins as it did in application to igneous rocks, although it had a definitely discouraging effect on the Wernerian theory. The result was, that students of ore deposits were left without a theory in which they could confidently repose. Werner's theory was discredited, while Hutton's was doubted, and certainly carried no weight among continental workers on ore deposits.

In these circumstances, there was little enthusiasm displayed in the study of ore deposits during the early part of the nineteenth century, during the first quarter of which very little was published on this subject. It seems clear, however, from such reports as are available, that much active thinking was then going on, coupled with a tendency to recognize the possibility of modes of origin other than those postulated by Werner and Hutton, and a desire to test these possibilities by a fuller and more careful study of field evidence.

Mention has already been made of the active speculation concerning the genesis of ore deposits that took place pre-

vious to and during the eighteenth century, before Werner's influence became effective. Prominent among these speculations was the theory of Descartes that metalliferous veins owe their origin to plutonic vapours and solutions generated in the earth's interior. It was to these speculations that thoughtful investigators of metalliferous veins now turned their attention as reasonable alternatives to the views of Hutton and Werner. There was thus a definite tendency, early in the nineteenth century, to broaden the basis of the theory of ore deposits. Neptunism, indeed, in its extreme form, soon lost its popularity even as regards metalliferous veins, and although controversy was still rampant, the disputants were chiefly plutonistic, the main point in dispute being whether dry magmas, or vapours and solutions escaping from wet magmas, had been the depositing agents.

It should be pointed out that the theory of Descartes had been kept well in view by various Continental authors, even during the period of Werner's supremacy. Thus, in 1788, Beroldingen had explained the origin of quicksilver deposits as due to sublimation from plutonic vapours (*Bemerkungen auf einer Reise durch die Pfälzischen Quecksilber-Bergwerke*). In his *Geologische Beschreibung des Thüringerwalds* (1806), J. L. Heim dealt, among other things, with the alteration products formed in various rocks by the sublimation of metalliferous and other matt given off by igneous eruptives. He attributed the formation of cellular secondary dolomites to this cause, as also did Von Buch at a later date (1822), when explaining the formation of the Fassa valley dolomites in the Tyrol as due to the sublimation of magnesia-bearing vapours from the melaphyre intrusives.

In this connection, and as showing how largely the notion of igneous vapours figured in the thoughts of some authors at that time, mention may be made of a paper by Cordier in which he showed that alunite had been formed by the action of sulphurous vapours on felspars (Ann. des Mines, 1820, vol. 5, p. 303). Also in this connection, it is noteworthy that Haüy, in his *Traité de Minéralogie* (1822),

included in his rocks a class of sublimation products, among which were sulphur and hæmatite.

Another important notion that secured some recognition in the early part of the nineteenth century, was the part played by water as an agent in inducing fluidity in lavas, and in volcanic phenomena generally.

Dolomieu (*Travels in the Lipari Isles*, 1783) had formed the opinion that heat alone was insufficient to induce fluidity in magmas, and inferred that it must be some special constituent of the magma, possibly sulphur, which gave a high degree of mobility to molten rock, and which, by its expansion, produced the scoriaceous texture of volcanic rocks.

In his *Introduzione alla Geologia* (1811), Scipio Breislak gave a lengthy account of volcanic phenomena and volcanic rocks in Italy, including some observations on gaseous eruptions. Although Breislak was a Plutonist, he was not one of the dry-magma school. He took up a middle position between the Huttonian and Wernerian schools, pointing out that the order of crystallization in granite, and the enclosures of liquid sometimes found in the quartz, rendered it difficult to regard the fluidity of granite as due to simple heat-fusion. He thought it more reasonable to attribute the formation of igneous rocks to the joint action of heat and water, rather than to the action of either of these separately.

In 1822, J. G. L. Schmidt (Karsten's Archiv f. Berg., B. IV), who did a considerable amount of work on ore deposits, expressed the opinion that hot waters, rising from the earth's interior, with mineral matter in solution, deposited their burden of mineral matter in fissures and cavities on their way to the surface in consequence of falling temperature :—

"Die damalige, durch Gährungen im Innern erhöhte Temperatur der Erde und der gesäuerten Flüssigkeiten, welche die Felsmassen nach allen Seiten durchzogen, begünstigte die Auflösung zerstreuter metallischer (und anderer) Stoffe, und es wurden dieselben den Gangspalten, und den noch nicht erhärteten, aus der Tiefe gekommenen, Gangmassen zugeführt, womit sich die infiltrirten Theile mannifaltig (mengten und) verbanden."

Another investigator who emphasized the significance of joint igneous and aqueous action was the geologist Ami Boué, whose *Mémoire géologique sur l'Allemagne* (Journal de Physique, 1822) included some interesting observations on igneous dike rocks and metalliferous vein rocks as a whole, to which he referred as *filons stériles et métallifères*. After a careful study of the origin of these rocks, and much investigation in mining districts, he reached the conclusion that, although Werner's theory was inadequate to account for the origin of metalliferous veins, aqueous action as well as igneous action seemed to be necessary to explain their origin.

"Après avoir ainsi balancé les probabilités en faveur des deux opinions, je crois qu'il y a plus de probabilité de voir dans certains filons des dépôts ignés associés avec des dépôts aqueux, que d'attribuer ces derniers à l'eau seule; et la différence de l'époque et des circonstances sous lesquelles ces sublimations ignées ont eu lieu, doivent expliquer pourquoi les dépôts trachytiques ne nous présentent guère de trace de dépôts semblables, et pourquoi les couches ignées en activité n'en exhalent qu'en très petit nombre" (*Op. cit.*).

In the previous year, 1821, Boué had already committed himself to a somewhat similar compromise in his *Essai géologique sur l'Ecosse,* in which he questioned the sufficiency of heat and pressure alone to account for the effects of metamorphism, suggesting that gases and vapours had played a prominent part in deep-seated metamorphism.

The problem of the cause of fluidity in lavas was discussed by G. Poulett Scrope in his *Considerations on Volcanoes* (1825). He agreed with Dolomieu that something other than heat was required to explain the fluidity of molten rock, but thought that water, and not sulphur, was the constituent responsible. With reference to this hypothetical constituent, which induced fluidity, he remarked that

"there is every reason to believe this fluid to be no other than the vapour of water, intimately combined with the mineral constituents of the lava, and volatilized by the intense temperature to which it is exposed when circumstances occur which permit its expansion" (*Op cit.*, p. 25).

Scrope's interest in magmatic water, however, as well as his interest in magmatic differentiation, was confined to the volcanic aspect of the problem, and did not extend to vein rocks or ore deposits, in which rocks he appears to have taken no interest. This applies also to Sir Charles Lyell at about that time; for although Lyell, in his *Principles of Geology* (1833), adopted Scrope's view as to the rôle of water in igneous magmas, like Scrope, his interest at that time was limited to the igneous aspect of the problem, and he gave no attention in the first edition of his *Principles* to the sulphidic vein rocks to which Hutton had attached so much significance.

Except with regard to its bearing on vulcanicity, indeed, the possible significance of the rôle of water as a constituent of volcanic lavas seems to have escaped recognition at that time, and Scrope's account of it appears to have made no impression on those of his contemporaries who were interested in vein deposits. This may, perhaps, be accounted for by the fact that the younger and contemporary volcanic rocks, in which he was interested, were regarded as of no importance to the student of ore genesis, as compared with the older igneous rocks, of deep-seated origin. It is difficult to believe, however, that Scrope's view of this matter escaped the notice of Élie de Beaumont, who, at a much later date, emphasized the significance of water as an agent in igneous action, but made no reference to the many earlier workers who had expressed similar views. Even Scheerer, who also at a later date, and about the same time as Élie de Beaumont (1847), claimed that water was an important constituent of granitic magmas, and extended its significance to the genesis of metalliferous veins, made no special reference to older individual workers, although he did admit in a general way that older geologists had held similar views.

CHAPTER VIII

NINETEENTH CENTURY (SECOND QUARTER)

NECKER, TAYLOR, FOURNET, FOX, LYELL, MURCHISON, DE LA BECHE, DARWIN, SCHEERER, DE BEAUMONT AND DUROCHER

A. L. NECKER

AN interesting and important paper on the origin of ore deposits was submitted to the Geological Society of London in 1832 (Proc. Geol. Soc., vol. I, 1826-1833, p. 392) by A. L. Necker. This paper was entitled *An attempt to bring under general geological laws the relative position of metalliferous deposits, with regard to the rock formations of which the crust of the earth is formed.* Necker had been strongly impressed by Ami Boué's views on the relative position of metalliferous veins and primary unstratified formations, as indicating that the metals had been deposited in the former by sublimation from the latter. He remarked that he had been led to believe in the sublimation of the metalliferous contents of veins from igneous matter, twelve years previously, by observation of the occurrence of specular iron-ore on the lava crusts of Vesuvius.

Necker also referred to the views of Von Humboldt, who, in his *Essai de Géologie et de Climatologie Asiatique,* accounted for the association of the mines of the Ural and Altai mountains with granite, porphyry, and syenite, by assuming that ore deposits were due to igneous action. Pointing out the association of great mining districts with occurrences of unstratified rocks, Necker referred to the

metalliferous porphyries of Mexico. He also took as a general illustration the absence of igneous rocks and metalliferous veins in the Mesozoic and Tertiary strata occurring in the large area between the Alps and western England, and the appearance of metalliferous veins in association with the unstratified rocks of Cornwall.

Necker emphasized the importance of this connection between igneous rocks and ore deposits as a guide to miners. He stated that ores were more abundant in granites, certain porphyries, syenites, etc., which he termed " underlying unstratified rocks," than they were in the newer porphyries, dolerites, and true volcanic formations, which he termed the " overlying unstratified rocks."

Some of the remarks made by Necker on the remoteness of igneous occurrences in certain districts where metalliferous veins are found, bear a striking resemblance to remarks made on this topic by modern advocates of the igneous theory. As instances of ore deposits entirely disconnected from unstratified rocks, he mentioned quicksilver veins at Idria, and galena veins in the Mountain Limestone of England. He mentioned other instances where, as in the Lead Hills of Scotland (Wanlockhead), and the metalliferous veins of the Vosges, although unstratified rocks are not present in the immediate vicinity of the veins, they are to be found some distance away; but he seems clearly to imply in such cases, as indeed is implied in the modern igneous theory of ore deposits, that the material of the veins was generated by igneous intrusions.

Necker appears to have had little to say in the abovementioned paper about the exact process of ore deposition, apart from the inference that it was one of sublimation. He was chiefly concerned to point out the close association that seemed to exist between intrusive igneous action and ore deposition, and to infer that the igneous action stood, in relation to the formation of metalliferous veins, as cause to effect. His paper seems to have had much influence in determining the views adopted by Fournet on the origin of

ore deposits. There can indeed be little doubt that it played a significant part in promoting the growth of the conception that intrusive igneous action is responsible for the origin of metalliferous veins, a conception which strengthened as time went on, although there continued to be much controversy as to whether aqueous and igneous agencies had acted co-operatively or separately in ore deposition.

J. TAYLOR

A paper worthy of note, entitled *Report on the state of knowledge respecting mineral veins,* by John Taylor, F.R.S., Treasurer of the Geological Society of London and the British Association, was read at the Cambridge meeting of the Association in 1833 (Rept. Brit. Assoc., 3rd Mtg., 1834). In this report, Taylor complained that the study of vein formation was at that time being neglected by geologists, and pointed out the scientific importance of the subject, claiming that " a better knowledge of veins generally must very materially contribute to sound investigation as to the structure of the rocks that inclose them," and that the inquiry was one " of practical utility, in which mankind are universally and largely interested."

Referring to Werner's claim to the discovery of the relative age of different sets of veins as indicated by one set cutting another, he stated that priority for that discovery properly belonged to Dr. Pryce, who, in his *Mineralogia Cornubiensis* (1778), pointed out that the east and west veins in Cornwall were antecedent to cross veins.

Taylor counselled moderation of opinion as regards ore genesis, and wrote in strong terms of the unreasonable attitude of the extremists, especially the Plutonists, who, according to him, were guilty of " great want of knowledge and many errors, arising out of the attempt to make all bend to a single method of solving the problem. . . . Our present state of knowledge as to the formation of veins should therefore, in my opinion, be allowed to admit that most of the causes which have been stated have operated at various

periods and through a long succession of time, some prevailing at one epoch, and some at another, modified by circumstances which we can but imperfectly comprehend or explain. . . . The action of water may, I think, be as fairly assumed as that of fire; and we may consider what their joint powers might be, when compelled, as it were, to act together, under circumstances that immense pressure might produce." (*Op. cit.*)

As indicating that many observers were then inclined to favour the sublimation hypothesis, it is of interest to note that Taylor, after remarking on the difficulties involved in the fusion and solution hypothesis, pointed out the possibility of origin from below by sublimation. He remarked that this hypothesis, which had gained many supporters, was far from new, as it had been held long before by Henkel and others that vein deposits were due to the exhalation of metallic vapours from deep-seated sources. On this question, while he allowed that sulphides could be volatilized in this way, he mentioned as a difficulty the occurrence of sulphides as isolated masses surrounded by matter that could not have been sublimed.

Taylor made an excellent critical survey of the views prevalent at that time, and showed much moderation of judgment in the account he gave. He presented Necker's views very fully and fairly as those of a sublimationist. He regarded it as a weak point in Necker's theory that, in many cases, there was no evidence of connection between metalliferous veins in stratified rocks and the underlying unstratified rocks, and on this point referred particularly to the Great Limestone bed of the north of England (Alston Moor), with its rich concentration of ore. He considered the evidence provided by this and similar instances to be directly opposed to the igneous injection theory; but thought that, with certain qualifications, its explanation by deposition from solution or by sublimation from vapours was more reasonable.

J. FOURNET

One of the most active observers on ore deposits during the 2nd quarter of the nineteenth century was J. Fournet, Professor of Geology at the University of Lyon, who contributed a very excellent section on *Etudes sur les Dépôts Métallifères* to vol. 3 of A. Burat's edition of D'Aubuisson de Voisin's *Traité de Géognosie,* published in 1835. It is curious that Fournet, who later became such a strong advocate of dry ore-magmas, and dry magmas generally, should be found figuring as a contributor to a work by D'Aubuisson, who had been one of Werner's favourite pupils, and who was regarded as one of the most authoritative exponents of his teaching. D'Aubuisson, however, issued only the first of the three volumes of the second edition of his *Traité* in 1828. Wishing to see the work completed, and the newer results incorporated, Burat assumed responsibility for vols. 2 and 3, which were published in 1834 and 1835 respectively, and invited Fournet to contribute a section on ore deposits for vol. 3. Fournet's contribution was also published separately in 1834.

D'Aubuisson himself, like other French workers who had been inspired by Werner, was much alive to the importance of geognostic considerations in the study of ore deposits, a subject in which, during the early years of the century, there had been a pronounced slackening of interest. With reference to this waning of interest in the geology of ore deposits, Burat made the following remarks by way of introduction to Fournet's *Etudes*:—

> "Malheureusement cette important partie de la géognosie a été jusqu'ici un peu négligée, et il est temps de ramener la science au point de vue des applications utiles, dont elle n'aurait jamais dû s'éloigner."

In these studies on metalliferous deposits, Fournet was clearly already persuaded in favour of the theory that ore deposits were due to igneous action, although he was far from being the convinced ore-magma and dry-magma extremist that he became at a later date. The following extract will serve

to show that Fournet, at that early date, while having made up his mind quite definitely that descending water of low temperature was not worth reckoning with as an agent in ore deposition, was comparatively open-minded as to the significance of ascending waters and vapours, and that he seemed inclined to allow that water and vapours of igneous origin were agents of much significance:—

"En effet il fallait être à même d'établir clairement que les filons offraient des points de contact nombreux avec les grandes phénomènes d'injection, d'épanchement, de sublimation, de fusion, qui ont modifié si fortement l'ancienne croûte du globe; qu'ils se trouvaient fréquemment en relation avec les faits que présentent les sources minérales, dont l'origine est si incontestablement centrale, puisqu'elles-mêmes sont toujours en rapport avec de profondes cassures, et enfin il fallait retrouver parmi toutes ces actions si nombreuses une série de faits palpables ou visibles, qui pussent nous expliquer clairement, par la similitude de leurs produits, comment la nature avait pu opérer à chaque grande commotion qui a troublé momentanément son instable équilibre : c'est ce qui nous reste à développer, autant du moins que les connaissances acquises nous le permettront" (*Op. cit.*).

In the actual examples that he mentioned, he gave much prominence to instances illustrating the effects of igneous action, and emphasized the connection so often displayed between non-stratified rocks and metalliferous veins, as had been done previously in papers by Boué and Necker already referred to. He included in his studies a separate section on contact deposits, in which he gave reference to papers by various of his contemporaries who had described contact metalliferous deposits, and who had indicated that such deposits owed their origin to igneous intrusions. Among these were M. Voltz (*Aperçu de topographie minéralogique de l'Alsace,* 1828), who had observed the similarity between fluor-barite-calcite-galena veins at Badenweiler and corresponding veins on the other side of the Rhine, both of which groups were near granite contacts; M. de Bonnard, who in 1823 had made observations on the lead-zinc veins in the west of France; and Elie de Beaumont, who, in his *Description des montagnes de l'Oisans,* had described metalliferous deposits containing galena, blende, copper-

and iron-pyrites, barite, and carbonates at the contact between stratified rocks and intrusive granites.

With reference to the last-named occurrence, Elie de Beaumont remarked that

> "D'après la manière dont ces substances sont disposées, il paraît qu'elles se sont insinuées dans une solution de continuité qui aurait existé entre le granite et les roches stratiformes, et sont venues en souder ensemble les deux parois, ainsi que celles de toutes les fentes qui y aboutissaient."

Fournet credited Delius with having, in the previous century, recognized as significant the occurrence of vein deposits along surfaces of contact between granite and limestone, and expressed regret that geologists had lost sight of this important fact and its significance :—

> "Il est fortement à regretter que les géologues aient perdu de vue cette indication précieuse et importante, en ce qu'elle les conduisait immédiatement à reconnaître à quel point le phénomène de la production des filons était dépendant de celui de l'injection des roches non stratifiées; rapprochement auquel nous voilà tout récemment revenus après d'immenses détours et après avoir épuisé, pour ainsi dire, la somme de toutes les observations possibles."

It is curious that, although Fournet in his *Etudes* gave quite a lengthy account of the views of older workers, including Werner, he made no reference to Hutton, who had stated the igneous injection theory of the origin of metalliferous veins in such a very definite way. As already mentioned, however, it was from a consideration of the mode of occurrence of vein deposits in relation to that of igneous intrusives, as pointed out by Necker, rather than from a consideration of the analogy between metalliferous veins and igneous dikes and the purely petrographic argument of Hutton, that Fournet was first led to adopt the igneous injection theory of the origin of veins; and if he knew of Hutton's views on this subject, he chose not to mention them.

While Fournet, however, in his *Etudes* showed such strong leanings towards a belief in a direct genetic connection between igneous intrusions and metalliferous veins, he

stated his views with much reserve, and showed clearly that he regarded this hypothesis as a merely tentative one, requiring much investigation before it could be definitely accepted. In a sense, therefore, he builded better than he knew; for there can be no doubt that the excellent account of the origin of metalliferous deposits given by him in these studies had a very important formative influence on European opinion as a whole, and that it did much towards the firm establishment of the view that water and vapours arising from igneous intrusives were of paramount importance in ore-deposition.

It was this view that dominated Burat's *Théorie des Gîtes Métallifères,* published in 1845, which received the blessing of Alex. Brongniart, Elie de Beaumont, and Dufrénoy in a favourable report on this work made by them as Commissioners to the French Academy of Sciences; and it was clearly the predominant view among eminent French authorities at that time. It was the view presented by Elie de Beaumont about that time in College lectures delivered by him in Paris, and embodied at a later date in an important paper by him published in the *Annales des Mines* (1847, see below).

While this view of things was being advocated by eminent authorities in Paris, Fournet himself, as the result of his field-work in southern France and elsewhere, was becoming convinced that metalliferous veins, like ordinary igneous dikes, were due to injections of magma rather than to deposition from aqueous solutions and vapours. This Huttonian view was also advocated in Germany by Petzold, who, in his *Geologie* (Leipzig, 1845), expressed the opinion that metalliferous veins were all magmatic injections, and that they were ramifications from the earth's metallic interior.

Fournet's views were all the more entitled to respect and consideration on account of the slow deliberation through which he reached them, the large amount of field-work they involved, and the fact that he started his career as a believer in the ascending hot solution theory. In his *Essai sur les filons métallifères du département de l'Aveyron* (Ann. Soc.

Agric. Lyon, 1844, vol. 7, p. 1; 1845, vol. 8, p. 90), he gave an account of the vein deposits of the Villefranche and Milhau districts of Aveyron in relation to the intrusive igneous rocks. He distinguished three petrologically distinct types of ore-veins in this district, namely, (1), a porphyry group characterized by fine-grained argentiferous galena and sugary quartz, with accessory barite, bournonite, chalcopyrite, blende, calcite and chalybite; (2), a serpentine group, characterized by hyaline quartz, chalybite, and calcite, with smaller amounts of galena, chalcopyrite, and bournonite, the other minerals being found only as accessories; (3), another serpentine group, characterized by granular quartz, with much barite and galena poor in silver, and smaller amounts of blende, bournonite, chalcopyrite and calcite.

In spite of the many features these different vein groups had in common, notably the occurrence throughout of bournonite, Fournet thought the differences sufficed to justify the view that they were genetically distinct, and that each of the vein groups was to be correlated with a distinct system of igneous intrusions. He claimed that the texture and disposition of these metalliferous veins showed that they were just as much eruptives as the ordinary igneous rocks with which they were associated. This agreed with his observations in other parts of France, as well as in the Alps and Tuscany, where he had studied occurrences of metalliferous veins in close association with granite, quartz-porphyry and serpentine intrusives.

A German translation by Von Cotta of Fournet's *Essai* on the Aveyron vein groups was published in 1846 under the title of *Die Erzgänge und ihre Beziehungen zu den Eruptivgesteinen*. In this translation Von Cotta admitted that, in many cases, metalliferous veins were closely associated with igneous intrusions. He quoted an early statement of his own, dated 1835, which he described as a fanciful speculation of his youth, in which he had declared that

"gewöhnlich stehen Erzgänge mit plutonischen Massengesteinen

in näher Beziehung, d.h. sie finden sich am haüfigsten, wo die gangführenden Gebirge von Porphyren oder ähnlichen Gesteinen durchsetzt sind."

Although, however, Von Cotta had repeated that statement in the same general way on several occasions, he admitted that he had never sought to prove it by bringing facts of observation to support it in the way Fournet had done. Moreover, while Von Cotta thought that igneous action was important in ore deposition, he was strongly opposed to Fournet's view that metalliferous veins consisting predominantly of such minerals as quartz, calcite and barite, which veins often showed symmetrical banding of the minerals, were due to magmatic injection. While allowing that ore deposits in some instances were probably of magmatic origin, he thought that, for such vein systems as those of Aveyron, it was better to invoke the action of hot mineral waters and vapours, as Bischof and Von Herder had done.

In a connected series of papers published at a later date in the *Comptes Rendus,* Fournet again dealt with the origin of ore deposits as a whole. (*Aperçus rélatifs à la théorie des gîtes métallifères,* C.R., 1856, vol. 42, p. 1097; and *Aperçus rélatifs à la théorie des filons,* C.R., 1856, vol. 43, p. 345, p. 842, p. 894). These papers, in which he restricted the processes of ore deposition and alteration to those of magmatic injection and atmospheric action, are of considerable interest as embodying his mature views on this subject. Summarizing his views in the final paper he wrote:—

"En résumé, l'organisation complète des filons me conduit à la conclusion qu'ils sont le résultat de deux causes, savoir: les actions plutoniques qui ont opéré par la voie de la fusion, et les actions atmosphériques qui ont remanié les produits antérieurs. La fusion, aidée de la pression, de la surfusion, de la cristallisation et de quelques effets mécaniques, peut expliquer tous les phénomènes de gîtes. La théorie que j'admets a d'ailleurs l'avantage de raccorder la formation des filons avec celle des roches éruptives. Elle explique parfaitement les transitions insensibles qui unissent ensemble les filons à silicates à ceux dont les gangues sont purement salines ou quartzeuses. J'attends, d'ailleurs, des objections autres que celles qui m'ont déjà été posées

pour y répondre d'un seul coup. Les actions atmosphériques ou superficielles sont trop naturelles, trop bien en rapport avec les principes chimiques, pour ne pas être acceptées. Elles généralisent les effets de la kaolinisation en les faisant passer du domaine des roches siliceuses à celui des matières filoniennes. Par leur caractère, ces deux théories satisfont plus que toutes les autres, au grand principe de Newton : Natura simplex est, et superfluis non luxuriat causis."

Even at the height of his career, however, and in spite of his extensive knowledge and field experience, Fournet practically stood alone as an advocate of the magmatic injection hypothesis, so little weight did his later views carry among his contemporaries. In France, he had opposed to him such influential authorities as Elie de Beaumont, Burat and Daubrée, with whom the eminent Von Cotta, who was very influential in Germany, was in agreement. It is hardly surprising that, in these circumstances, when Sir Charles Lyell, who had not previously shown any serious interest in this subject, came to refer to it in his Presidential Address to the Geological Society in 1850, all he did was to mention with approval the views of Elie de Beaumont. Indeed, at that time in England, apart from the work of Phillips, very little work had been done on ore deposits; and such views as were held by geologists were doubtless for the most part influenced by the belief, which had been so strongly advocated by Scrope, and which had been confirmed by the petrographic work of Sorby, that water was an important constituent of igneous magmas. It was doubtless the close agreement between this belief and the views held by Elie de Beaumont and Scheerer, that led Lyell to favour these views.

R. W. FOX

In a note *On the Formation of Mineral Veins* (Proc. Geol. Soc., 1833-38, vol. 2, p. 406 and p. 540) R. W. Fox stated the view that the deposition of minerals in veins was due to differences in electrical conditions between the opposite walls of the fissures. In consequence of this, currents which were set up in the solution occupying the fissures led to deposition of material held in solution. The increase

in productiveness of veins in passing from one formation to another he attributed to the electronegative conditions of the rocks enclosing the richer parts of the veins.

Such vein deposits might in some measure be derived by infiltration from the enclosing rock; but he thought that they could be derived in almost indefinite quantities from hot solutions ascending from the deeper parts of the fissures in which they were formed. Such hot solutions become highly charged with earthy and metalliferous matter, which would be deposited in the upper and cooler parts of the fissure, through fall in temperature; but he regarded electrical conditions as chiefly responsible for deposition, especially the preference for deposition in one rock rather than in another.

C. LYELL

Lyell's attitude towards the study of ore deposits exemplies very well the indifference of most English geologists on this subject during the first half of the nineteenth century. In the first edition of his *Principles* he ignored the subject altogether. In the first edition of his *Elements of Geology* (1838), he referred only in the most casual manner to the observed occurrence of metalliferous matter at or near the junction of plutonic rocks with stratified formations, and he mentioned only Necker's sublimation view by way of explanation.

In the Presidential Address already referred to (Quart. Journ. Geol. Soc., 1850, vol. 6, p. lx), Lyell again mentioned the subject rather casually, making no reference to the work of Fournet and others. He had clearly given very little thought to the problem, and it was natural enough, in the circumstances, that he should join the big battalions by assenting, as he did in the following terms, to the views held by Elie de Beaumont, whose paper had only recently been published:—

"I fully assent to the doctrine so ably advocated by Elie de Beaumont, that a large class of metalliferous veins may simply be regarded as extinct mineral springs. They are fissures in which vapours or thermal waters charged with various elementary bodies,

have precipitated the materials of a refractory kind, or those which are least readily retained in solution. The marked agreement between the contents of mineral springs and the emanations from active volcanoes strongly supports this view. But why should we doubt that fissures now existing in solid rocks may in like manner communicate at one extremity with subterranean masses of fused matter, while at their upper end they terminate in mineral springs? And if so, why may not hot steam and gases and mineral waters be depositing at this moment, as actively as ever, that class of elementary bodies, whether metalliferous or not, which we find in the oldest veins? The steam or hot water will always part with these substances in the deeper parts of every fissure, and merely bring up to the surface the residuary salts which are more soluble and volatile. Hence mineral veins are marked by the habitual absence of alkalies, which are so readily dissolved in water."

Influential European opinion was thus definitely and overwhelmingly against the theory that had gradually matured in Fournet's mind as the long result of much patient thought and field-work. This magmatic theory, indeed, never got much of a footing in those days, and Fournet's scientific friends lamented the fact that he had abandoned the views set forth in his *Etudes,* which, as repeated with some elaboration by Elie de Beaumont, Burat and Von Cotta, continued in favour among the majority of European workers, and persists largely in the theory of ore deposits dominant at the present time.

H. T. DE LA BECHE

In his later writings, De la Beche took up a non-committal attitude on the problem of the origin of lodes. In his *Report on the Geology of Cornwall, Devon and West Somerset* (1839), however, he had a long chapter on the formation of mineral veins, in which he gave an account of the work of his contemporaries, including that of Fournet, but he explained the formation of veins by lateral secretion :—

"When we take a general view of the filling of the dislocations in the district, whether termed common faults, lodes, or cross courses, we see that it has depended upon conditions among which the mineral matter of the adjacent rocks holds a prominent place. Upon this character seems to have greatly depended the nature of the chief mineral

substances in them. Among the limestones we find carbonate of lime abundant, and among the siliceous rocks, quartz. We therefore infer either that water charged with matter derived from the adjacent rocks has infiltered into the fissures, or that the liquid contents have acted on the adjoining rocks and dissolved a portion of them.

Upon the mineral characters of the adjacent rocks seems also to have greatly depended the accumulation of the ores of the useful metals, it generally happening that when two or three dissimilar rocks are traversed by the same fissure they are most abundant."

R. I. MURCHISON

Reference having been made above to the indifference displayed by most English geologists towards the study of ore deposits during the first half of the nineteenth century, it would be unfair to avoid mention of the interest in this subject shown by Murchison and Darwin. In a paper *On the Geological Structure of the Ural Mountains,* by R. I. Murchison, M. E. de Verneuil and Count A. von Keyserling (Proc. Geol. Soc., vol. 3, 1842), a section is devoted to " igneous, metamorphic and metalliferous rocks." In this paper it was pointed out that practically all the gold mines of the Urals were on the eastern flank, and that it was on this flank also that the great masses of igneous rock occurred. In confirmation of views previously expressed by Humboldt, the authors state that there is an intimate connection between the eruption of the plutonic rocks and the formation of the gold deposit on this flank of the Urals. They state, moreover, that this intimate connection between mineral veins and eruptive rocks applies also to other metalliferous deposits on this flank of the Urals,

" for, with very rare exceptions, it is only on their eastern or eruptive side that copper veins, malachite, platinum and magnetic iron prevail."

C. DARWIN

In an account by Charles Darwin of the metalliferous vein deposits of Central Chile, given in his record of geological observations made in South America during the voyage of the Beagle (*Geological Observations in South America,* 1846), he stated that the trend of the veins was

governed by the structure of the rocks, and concluded his remarks on this subject as follows:—

"Finally, I may observe that the presence of metallic veins seems obviously connected with the presence of intrusive rocks, and with the degree of metamorphic action which the different districts of Chile have undergone. Such metamorphosed areas are generally accompanied by numerous dikes and injected masses of andesite and various porphyries; and I have in several places traced the metalliferous veins from the intrusive masses into the encasing strata. Knowing that the porphyritic conglomerate formation consists of alternate streams of submarine lavas and of the debris of anciently erupted rocks, and that the strata of the upper gypseous formation sometimes include submarine lavas, and are composed of tuffs, mudstones and mineral substances probably due to volcanic exhalations, the richness of these strata is highly remarkable when compared with the erupted beds, often of submarine origin, but not metamorphosed, which compose the numerous islands in the Pacific, Indian and Atlantic Oceans; for in these islands metals are entirely absent, and their nature even unknown to the aborigines."

This emphasis by Darwin on metamorphism as a factor in the genesis of metalliferous veins is a highly interesting and original feature. Previous writers had not emphasized metamorphism as a factor in the formation of mineral veins, though at a later date Daubrée and other workers attached great importance to it.

T. SCHEERER

In 1847, there appeared an important paper by T. Scheerer, entitled "*Discussion sur la nature plutonique du granite et des silicates crystallins qui s'y rallient*" (Bull. de la Soc. Géol. de France, 1847, 4, Pt. 1; Séance du 15 Fevrier, 1847). From the standpoint of the theory of ore deposits, the chief feature of Scheerer's paper was his view that water had played an important part in the formation of granite magmas. In support of his view that granite magmas owed their formation to "ignéo-aqueuse" fusion, he pointed to the hydrous character of the micas, to geodic cavities in the granite, and to the veins associated with granitic intrusions.

"On est convaincu que l'état primitif de fusion simplement ignée de cette roche (granite), quoique les phénomènes de contact soient en

faveur de cette hypothèse, n'est pas justifié par la nature intime de la masse granitique elle-même. . . . Les éléments du granite, ou du moins la totalité de ces élements, n'a donc pu se trouver dans la pâte à l'état d'hydrate car dans ce cas la quantité d'eau contenue dans la bouillie granitique n'aurait pu être moins de 50 pour 100, tandis que la quantité d'eau dont l'existence dans les granites peut être démontrée de nos jours, ne dépasse guère un à quelques pour 100. La quantité d'eau contenue primitivement dans le granite doit par conséquent être entre 1 et 50 pour 100, et il est très probable que ce quantum se rapproche davantage du minimum que du maximum."

Referring to the formation of mineral veins as evidence of the exudation of aqueous solutions from granite intrusions, he remarked :—

" Nous observons les mêmes phénomènes dans une certaine classe de filons qui se présentent dans les roches granitiques ou autres. On peut y reconnaître d'une manière, je dirai presque matérielle, que leur remplissage s'est faït par les dépôts de solutions ou de bouillies qui s'échappaient de la roche encaissante. C'est pourquoi on les a nommés filons de sécrétion."

At the time when Scheerer's paper was published, the view he advocated appears not to have been widely entertained. So far as metalliferous veins were concerned, the belief that they were formed by sublimation seems to have been widely held. As regards igneous rocks, the extremists of the plutonist school still held that, as granite had been strongly heated, it must have been dry and not aqueous; but Scheerer showed that this view of things was not in accordance with facts. While contending that water was an important constituent of granite magmas, however, he made no claim to originality for this idea. He pointed out that many geologists had already accepted the view that water played an important part in the formation of granites, and remarked :—

" C'est d'ailleurs un précepte ancien et reçu des plutonistes, qu'il faut se figurer les roches cristallines primitives comme ayant été fondues sous l'eau et sous une forte pression."

This remark presumably referred to views previously expressed by Breislak, Boué, Scrope and others, to whom reference has already been made.

ELIE DE BEAUMONT

Almost at the same time as Scheerer's paper came one by Elie de Beaumont entitled "*Note sur les émanations volcaniques et métallifères*" (Bull. de la Soc. Géol. de France, 1847, vol. 4, pt. 2, p. 1249, Séance du 5 Juillet, 1847), which may perhaps be regarded as the most important and influential paper ever published on the theory of ore deposits. Elie de Beaumont had long been a diligent worker at this subject, which he had studied in Paris under Brochant de Villiers, who was one of the most ardent of Werner's pupils and admirers.

Elie de Beaumont developed a keen interest in the study of ore-genesis quite early in his career, in spite of his heavy duties in connection with the production of the first geological map of France, and maintained this interest throughout his life, coupling it closely with his other geological work. Together with Alex. Brongniart and Dufrénoy, he acted on a French Academy of Sciences Committee which was responsible for reporting on papers by French workers on this subject. In this capacity, apart from much excellent original work of his own, he followed very closely and thoroughly the work of his contemporaries, and was probably in his day the most profound of the French authorities on the geology of metalliferous deposits.

Elie de Beaumont was one of the first to recognize the significance of the broad structural lines of the earth, as shown by the trend-lines of mountains and the relative disposition of rock formations. These he attributed to shrinkage in the earth's mass, and consequent crust movement. He formulated the view that the main lines of earth structure, as shown by the trend-lines of mountain chains, were due to catastrophic dislocation of the rocks of the earth's crust at different geological periods, followed by periods of quiescence and sedimentation. He held that the trend-lines or folds thus developed at any given period were parallel, and that the different systems showed a pentagonal arrangement.

It is perhaps worthy of mention that Elie de Beaumont caught his inspiration for this idea from the teaching of Werner, who taught that, as a rule, mineral veins, the trends of whose strike were parallel, were of the same age. It was this notion, gathered from Werner's teaching, that Elie de Beaumont applied in his study to the trends of mountain systems, with results that have proved of such far-reaching importance in the interpretation of the tectonic and other broad geological features of the earth by Suess and others.

In the paper referred to above, Elie de Beaumont showed clearly that he realized the scientific significance of metalliferous veins as constituting a petrological group meriting consideration along with igneous dikes as types of rock formed by the infilling of fissures in the earth's crust. Why the general run of petrologists and geologists should ever have taken a different view is difficult to understand, except as indicating a stronger taste on their part for expediency and the avoidance of difficulties than for the integrity of their science. At any rate, such papers have received little or no notice by the historians of geology and petrology, having been regarded as insignificant from the viewpoints of those sciences.

In this paper, Elie de Beaumont distinguished two kinds of volcanic products, namely, lavas and sublimates :—

> " On peut donc distinguer deux classes de produits volcaniques, ceux qui sont volcaniques à la manière des laves, et ceux qui sont volcaniques à la manière du soufre, du sel ammoniac etc. . . . Je désigne l'ensemble de ces produits par la dénomination d'emanations volcaniques et métallifères, parce que la plupart des filons métalliques me paraissent s'y rapporter. Il faut même y comprendre un grand nombre de gîtes de minéraux pierreux."

He claimed, however, that in no case of vulcanicity was there any evidence of dry sublimates. On the contrary, there was evidence of an abundance of water vapour, which was an essential feature in the mechanism of volcanic activity.

In connection with volcanic activity, he distinguished between the action of volcanic vapours and the action of

thermal springs. The former gave rise to sulphur, alkaline and metallic chlorides, etc.; while the latter gave calcareous and ferruginous deposits when the springs were of low chemical activity, whereas if they were charged with more active agents they yielded deposits of silica or complex mixtures of barium, boron, arsenic, sulphur, fluorine and other products. It was to the operation of such active solutions, derived from igneous sources, that he attributed the formation of metalliferous veins, which had been formed by deposition on the walls of fissures, and which must on that account be sharply distinguished from veins such as those of porphyry and basalt, which had been injected in a molten condition. The former he termed *filons concrétionnés,* the latter *filons injectés.*

He pointed out that, although metalliferous deposits occur usually in the form of veins, they are found also as segregations in the igneous rocks themselves, in which they have crystallized out during the cooling of the intrusive mass. As instances of these he mentions occurrences of magnetite, chromite and platinum as segregations in basic igneous rocks such as those of Taberg in Sweden, the Urals and other areas. He also gives examples of contact deposits of magnetite, pyrites and other minerals.

The fact that some minerals are segregated in the igneous rocks themselves, whereas others are carried off as emanations into or through the enclosing rocks, he attributes to differences of chemical properties among the metals concerned:—

"La diversité de propriétés chimiques des différents métaux permet donc de concevoir assez aisément pourquoi le platine et les métaux qui l'accompagnent sont presque uniquement concentrés dans les roches éruptives qui les récèlent, tandis que le fer, le cuivre, l'argent, le plomb, se sont répandus dans les masses au milieu desquelles les roches métallifères ont fait eruption et s'y sont répandus souvent jusqu'à des distances considérables."

From the fact that the metals occur in the eruptive rocks as well as in the rocks invaded, he considered it difficult to doubt that the metals had been introduced by the intrusive.

This he regarded as being rendered the more probable by the fact of the concentration of the metals often observed in the igneous intrusions themselves, especially along contacts.

He held that a complete examination of a series of metalliferous deposits such as was often found in close proximity to eruptive rocks, showed that there was an intimate connection between the two, and that a gradation was traceable from magmatic deposits in which the metalliferous minerals had crystallized (i.e., segregated) in the igneous rock, to veins in which similar minerals had been deposited by incrustation in fissures.

He remarks that many geologists of his time regarded vein deposits as magmatic intrusions, but he combats thi: notion :—

> "Beaucoup de géologues sont portés à admettre que touts les filons ont été remplis par l'injection de matières en fusion. . . . Si les matières qui remplissent un filon y avaient toujours été injectées à l'état de fusion, comment expliquerait-on, par exemple, un filon composé de bandes alternatives de fer spathique et de quartz?
>
> Au contraire, l'hypothèse qui attribue les filons métalliques ordinaires à des émanations sous forme de vapeurs ou d'eaux minérales permet de concevoir les faits les plus variés que présentent les filons, et, par exemple, le développement des affinités chimiques, dont on a remarqué depuis longtemps l'influence dans la manière dont les métaux y sont associés. Les substances qui y sont généralement réunies ont beaucoup de rapport entre elles, et souvent même des propriétés tout à fait analogues."

As examples of these associations, he mentions nickel and cobalt, antimony and arsenic, silver and lead, lead and zinc as sulphides, and the various associated metals of stanniferous veins.

Elie de Beaumont attributed the difference between granite and the younger (volcanic) igneous rocks to the difference in the materials present when the rocks were in a molten condition. Granites contained mineralizers, which they gave off before crystallizing. He attributed the difference between such deposits as platinum and those of tinstone partly to differences in affinity for oxygen and partly to differences in specific gravity. He regarded the phenomena

of igneous intrusion and the accompanying emanation effects as a process of natural cupellation. Silicon and potassium especially he mentioned as elements liable to oxidation.

He also attached much importance to mineralizers as agents by which metalliferous matter was carried off by emanations escaping from igneous intrusions. On this subject he remarked :—

> " L'aspect métallique de la plupart des combinaisons des métaux avec les minéralisateurs et la ressemblance que cet aspect leur donne avec les produits des opérations métallurgiques, semble autoriser la supposition que ces combinaisons sont dues à la seule action de la chaleur, et cette supposition semblerait être confirmée par l'hypothèse qui attribue au minéralisateurs le rôle d'agents de volatilisation."

As regards the origin of thermal waters, Elie de Beaumont thought that, in most cases, they could not be adequately explained as being of surface origin ; on the contrary, it seemed to him highly probable that they were as a rule derived directly from igneous intrusions :—

> " Il est probable que les sources thermales les plus chaudes, les sources thermales principales, émanent directement des roches éruptives."

He adopted Scheerer's view that a granite magma was an aquo-igneous rather than a simple igneous fusion, and agreed with Scheerer that granite magmas, at the time of intrusion or eruption, probably do not contain more than two or three per cent. of water. In this connection, he pointed out how the presence of quite small quantities of elements, such as carbon, silicon, phosphorus and manganese, affected the crystallization and properties of steel, and thought it reasonable to infer that the presence of a small quantity of water in a granite magma at the point of crystallization must have had a pronounced effect on the crystallization process. As regards the effect of mineralizers generally, he remarks :—

> " De là je serais porté à conclure que le composé volatil renfermé dans le granite avant sa consolidation, contenait non seulement de l'eau, du chlore, du soufre, comme la matière qui se dégage des laves

lorsqu'elles se refroidissent, mais qu'il contenait en outre du fluor, du phosphore et du bore, ce qui lui donnait beaucoup plus d'activité et la faculté d'agir sur beaucoup de corps sur lesquels la matière volatile contenue dans les laves n'a qu'une action comparativement insignifiante. . . . La présence de ces substances paraît avoir eu pour effet de suspendre la cristallisation du granite, et de la suspendre jusqu'à un refroidissement d'autant plus avancé qu'elles étaient plus concentrées. Les granites n'ont cristallisé qu'après qu'elles étaient dissipées ou fixées; mais lorsqu'ils ont été mis en contact avec des corps froids, après que ces substances avaient disparu, ils n'ont plus résisté aussi énergiquement à leur action coagulante, et ils se sont immédiatement consolidés sans que leur grain cristallin ait pu se développer. Ils se sont alors conduits comme presque toutes les autres roches éruptives."

He admitted that ore minerals are often deposited among sedimentary beds laid down in surface waters, as for example the Permian copper deposits of the Urals, the cupreous shales of Thuringia, calamines, manganese and iron ores, beds of gypsum and dolomite, strontium sulphate associated with sulphur in Sicily, and strontium sulphate in the gypsum deposits of the Paris basin. Even in these instances, however, he appears to have regarded the metalliferous material as having been derived from solutions which reached the surface from igneous sources, having traversed fissures without depositing their entire load of dissolved mineral matter. Thus he concludes that all mineralizing waters are of deep-seated igneous origin, and not as Werner had supposed, of superficial origin :—

"On admet avec Werner que les substances minérales ont été déposées par l'action des eaux, que les filons ont rempli les fentes; mais on n'admet pas que ç'ait été par des dissolutions superincombantes: on admet, au contraire, que les substances répandues à la surface sont venues de l'intérieur de la terre; qu'elles ont été entraînées, soit par eaux minérales, soit quelquefois par des vapeurs aqueuses; qu'elles ont été déposées en partie dans les fissures par lesquelles ces émanations passaient, et que le reste seulement de ce qui a pénétré dans les fissures et s'y est en partie fixé s'est répandu dans les eaux superficielles, et a été finalement déposé par elles."

J. DUROCHER

J. Durocher at first strongly supported Elie de Beaumont's views against those of Fournet. Later, however, he

came to regard the irregular distribution of metalliferous and other minerals in lodes as proof that they had been formed by sublimation from gases or vapours (Comptes rendus, 1849, vol. 28, p. 607). Of these gases or vapours there were two kinds which he distinguished as *émanations motrices* (metallic) and *émanations fixatrices* (sulphurous). These gained access to fissures, sometimes separately, at other times joining their forces to produce more complex results.

CHAPTER IX

NINETEENTH CENTURY (THIRD QUARTER)

BISCHOF, VON COTTA, DAUBRÉE, BELT, STERRY HUNT AND WALLACE

BISCHOF

IN the three substantial volumes of his excellent *Lehrbuch der chemischen und physikalischen Geologie* (Band 1, 1847; Band 2, Abt. 1, 1851; Band 2, Abt. 2 & 3, 1855; Supplement-Band in 1871 by F. Zirkel), K. Gustav Bischof marshalled in very good style the facts then known concerning geochemistry, and sought to secure recognition for low-temperature aqueous processes, a rational view of which he regarded as the best way of accounting for the geochemical and geophysical data then available. In judging Bischof's efforts in this direction, it should be remembered that Werner's views naturally persisted longer in Germany than they did in France and England. There was, indeed, in Germany, about that time, a school of Ultraneptunists led by J. N. Fuchs, who denied the claims of Plutonists altogether, and contended that granite and other plutonic rocks had crystallized from an amorphous magma saturated with water, but not from a state of fusion. The existence of this school is of course well explained as a reaction to the school of Ultraplutonists, who denied the claims of the Neptunists altogether. In such controversies, one extreme generally begets another.

It is therefore only fair to Bischof to point out that he was not an Ultraneptunist, and that he was very reasonable and also very scientific in the claims he made for the recogni-

tion of the importance of low-temperature aqueous processes in connection with the genesis of rocks and ore deposits. He had given a great deal of attention to the mineralogical changes taking place in the earth's crust in connection with the action of meteoric waters and other low-temperature processes, and was convinced that the plutonists under-estimated the significance of these comparatively superficial phenomena in their accounts of rock genesis. No one who wished to do justice to the large body of facts concerning superficial processes could fairly have done otherwise at that time than depreciate the enthusiasm of the Plutonists, and in doing this in his *Lehrbuch,* Bischof put on record a vast amount of useful data in support of his case. If it be true to say that he over-emphasized the significance of meteoric waters and low-temperature processes, it is at least equally true to say that many of the Plutonists at that time grossly under-estimated their significance. He shows indeed in his *Lehrbuch* that he was not by any means an immoderate advocate, as can be seen from the following passage :—

"Diejenigen Geologen, welche, ausser den sedimentären Formationen, alles Uebrige zu den eruptiven Gesteinen zählen, welche die Gangmassen in den Erz-, Quarz-, Kalkspath-, Barytspath-, und Flussspath-Gängen, wie die in den Basalt-, Melaphyr-, Porphyr-, Granit-Gängen u.s.w., auf feuerflüssigem Wege entstehen lassen, sind wohl zu unterscheiden von denjenigen der plutonischen Schule, welche die Macht des Vulkan's bloss auf die Bildung der krystallinischen Gesteine beschränken. Jene, die wahren Ultraplutonisten, haben sich eben so auf das eine, wie die Ultraneptunisten, welche alles aus dem Wasser entstehen lassen, auf das entgegengesetzte Extrem gestellt. Est ist schwer zu sagen, ob sich diese oder jene mehr an der Natur versündigt haben.

Das juste milieu ist so leicht zu finden. Wer alles aus dem Wasser werden lässt, der erhebt sich über den Standpunkt der Wissenschaft; denn statt von dem Gegebenen auszugehen, von dem Bekannten auf das Unbekannte zu schliessen, schweift er in das Reich der Fictionen, er hält sich an das Mögliche und vergisst dabei das Wirkliche."

Bischof gave the weight of his great authority in support of the view already widely known as the lateral-secretion theory, a theory of ore genesis which was afterwards stated in a more extreme form by Sandberger. This view was in-

deed held by other workers contemporary with and previous to Bischof, but it is probably to Bischof that chief credit should be given for first stating clearly and fully, with adequate data to support his statement, the important fact of the dissemination of metalliferous minerals in the sediments and other superficial rocks of the earth's crust, and the significance of this in connection with ore genesis.

Modern opinion on ore genesis has tended to repeat the errors and excesses of the older plutonists on this question; but it seems probable that ultimately there will be a return to a more moderate view of things, in which a more ample allowance will be made for the operation of processes connected with sedimentation and low-temperature metamorphism and the effects of segregation resulting therefrom.

B. VON COTTA

The most eminent authority on the theory of ore genesis in Germany about the middle of the nineteenth century was Bernhard Von Cotta, who was a profoundly scientific worker and a leading authority on petrology. The fact that he coupled a very thorough study of petrology with an equally thorough study of ore genesis gave weight to his authority, and it was doubtless due mainly to his influence that the theory of the origin of metalliferous veins by ascending hot solutions (infiltration from below) owed its prestige in Germany.

Neptunistic views as advocated by Werner had already been abandoned by most German workers, especially after Von Beust's destructive criticism in his *Kritische Beleuchtung der Wernerschen Gang-theorie* (1840); and Von Cotta, in his *Die Lehre von den Erzlagerstätten* (1859), of the 2nd edition of which an English translation by Prime appeared in 1879, presented in very good style the magmatic-water infiltration theory, which had already been expounded so clearly by Elie de Beaumont and others in France. This treatise by Von Cotta was the standard work on the subject for at least twenty years. It was a well wrought work, care-

fully balanced in its treatment of the subject, and paid due regard to the complexity of the processes involved in ore deposition.

Von Cotta was not a dogmatist; and while he held firmly to the theory of infiltration from below, he realized that the then existing state of the evidence available gave no security of tenure to this theory, although it might be regarded as giving it a strong claim to probability. He pointed out that systems of classification, which carried such a strong appearance of completeness and definiteness, were really very arbitrary, and should not be allowed to mislead one into accepting dogmatically the speculations on which they were based. He remarked that, while certain deposits could be clearly and definitely placed in certain groups, there were others which seemed to unite the characters of different groups.

After a careful and critical consideration of the various hypotheses available for a general explanation of the origin of ore deposits, he came to the conclusion that such a general explanation, applicable to all deposits, was impossible of attainment. He held the view that metalliferous veins had arisen in various ways, and that every occurrence needed to be considered in the light of the special circumstances connected with it. He recognized the possibility of igneous fluid injection in some instances. In others he invoked sublimation. For veins showing a symmetrically combed arrangement he thought that, without doubt, they had been formed by gradual deposition at considerable depth from aqueous solutions of deep-seated rather than superficial origin; and this view he thought especially applicable to the Freiberg lodes and others like them.

He emphasized the value of Bischof's work as showing that dilute solutions, acting over long periods of time, were capable of depositing the material of mineral veins.

One of the important features of Von Cotta's work was his recognition of the zonal arrangement in depth of ore deposits. He realized that, if ore-bearing solutions were of

plutonic origin, and if some deposits were the direct products of deep-seated igneous action, then there must be various phases of ore-deposition conditioned by heat and pressure, which vary with depth. He explained in this way the differences between dissimilar zones of ore deposits, attributing them to the fact that the temperature and pressure conditions of deposition were different for the different metals.

He pointed out instances where, in certain districts, there was evidence of a downward succession of ore-zones from gold near the surface through a deeper zone of lead ore to a still deeper zone of copper ore, and explained this succession by the zonal theory above referred to.

He explained that the zone of ore actually exposed near the surface at any particular time was dependent upon the amount of denudation that had taken place since the deposit was formed. Where denudation had been extensive, veins of deep-seated origin may be found at the surface, whereas deposits of shallow origin could be expected at the present surface of the earth only where the deposition was of comparatively recent age, or where there had been very little denudation.

A. DAUBRÉE

The traditional recognition by French geologists of the importance of the study of ore genesis, established in the early days through Werner's influence by such workers as Brochant de Villiers and D'Aubuisson de Voisin, and carried on more especially by J. Fournet and Elie de Beaumont, was well maintained by A. Daubrée, who made some valuable contributions to the study of ore genesis, not only as a field observer, but also as a laboratory experimentalist.

Daubrée published the results of his experimental work in numerous scientific papers at various times during a long and productive scientific career; but for the most part he collected these results in his *Etudes synthétiques de Géologie expérimentale* (1879), some of which studies had a close bearing on ore genesis. As indicating the possible origin of

sulphides by the action of natural sulphidic waters, he showed that Roman coins long buried in mud were largely converted into chalcopyrite and other copper sulphides by the action of sulphidic waters. Under similar conditions, old lead pipes showed a development of galena. He observed that iron-pyrites was formed by the action of sulphuretted hydrogen on metallic iron in the Icelandic basalts, and that it could also be formed by the reducing action of decomposing organic matter on iron oxides.

In his *Mémoire sur le gisement, la constitution, et l'origine des amas de minerai d'étain* (Ann. des Mines, 1841, vol. 20, p. 65), he observed that fluoriferous minerals (fluorspar, topaz, etc.) were commonly associated with tin ore, as also were boron compounds, especially tourmaline; and he inferred that the minerals characteristic of tin-ore deposits, including wolframite and molybdenite, had been deposited from vapours containing fluorides and boron compounds of various metals. He regarded these vapours as having been generated at considerable depth, whence they ascended towards the surface through fissures, depositing their load of metalliferous matter partly as veins in the fissures, and partly as impregnations in the surrounding rocks.

To this action of the deposition of metals from fluoriferous and other vapours, the name "pneumatolysis" was applied by Bunsen, but it was Daubrée who first called attention to it in connection with the genesis of tin ores, and emphasized the significance of fluorine and other elements as carriers of the metals. It should be mentioned, however, that both Daubrée and Elie de Beaumont were contented with the name mineralizer (*minéralisateur*) for these highly active agents in ore deposition, and neither of them regarded it as simply a case of gas action. They both regarded water as an essential agent, and Daubrée used steam in his experimental production of artificial cassiterite from stannic chloride, which was more conveniently available than stannic fluoride. This point is especially worth mentioning, be-

cause Bunsen's term " pneumatolysis " has been applied so loosely that some authors have abandoned its use as a term of no scientific value.

Indeed, highly as he rated the significance of fluorine and other active elements, Daubrée attached far more importance to water, and it was to hydrothermal agencies that he attributed the chief rôle in metamorphism and ore deposition. He made a very thorough study of the circulation of water in the earth's crust, and wrote an important treatise on this subject in three volumes (*Les eaux souterraines à l'époque actuelle,* 2 vols., 1887; and *Les eaux souterraines aux époques anciennes,* 1 vol. 1887). In his account of underground waters, Daubrée included an elaborate study of their chemical composition, their thermal condition and the causes affecting their movement. While not denying that some subterranean waters may have been what is now known as *juvenile,* i.e., magmatic water derived directly from igneous intrusions, he thought they consisted chiefly of what is known as *vadose* water, i.e., water of atmospheric origin that had seeped into the crust from the surface.

Daubrée held that water of atmospheric origin thus penetrated to considerable depths by capillary permeation, in spite of the high temperature, and, after becoming heated, exerted an important influence in metamorphism. He was, indeed, of opinion that the chief factor in regional metamorphism and ore deposition at depth was the hydrothermal factor, due to the action of these geothermal waters of superficial origin.

Daubrée was, therefore, in close agreement with Elie de Beaumont as to the nature of the agents involved in the formation of metalliferous veins; but whereas Elie de Beaumont coupled their formation with the operation of igneous action simply, Daubrée linked it with the more widespread phenomena of hydrothermal metamorphism. He reached this conclusion after a long study of the geological relations between hot springs, metalliferous veins, volcanoes and earthquakes. He showed that, in the distribution of

the effects associated with these various groups of phenomena, there were close analogies and relationships, and he regarded this as evidence that they were different manifestations of the operation of deep-seated hydrothermal activity. His views on these matters are set out in his *Etudes et expériences synthétiques sur le métamorphisme et sur la formation des roches crystallines* (1860), as follows:—

"Si les sources thermales sont des agents du métamorphisme, il ne faut pas s'étonner qu'un même mode de métamorphisme s'étende sur des régions considérables, puisque nous voyons encore aujourd'hui les eaux minérales se grouper par familles de composition analogue, dans des régions d'assez grande étendue; ainsi elles sont généralement carbonatées dans l'Auvergne et dans l'Eifel, sulfureuses dans le Pyrénées, etc.

On rencontre ces analogies plus caractérisées encore dans les gîtes métallifères, qui paraissent être aussi un produit d'origine semblable, et bien que la plupart d'entre eux présentent de nombreuses espèces minérales, souvent même distribuées d'une manière très-inégale dans les diverses parties d'un même filon, la nature des gangues, aussi bien que celle des métaux, qu'il est possible d'y exploiter utilement, montre que généralement ils se groupent par systèmes. Ces systèmes embrassent quelquefois des régions considérables, surtout dans les continents dont la structure géologique n'est pas morcelée comme celle de l'Europe occidentale. (Exemples: les groupes argentifères du Mexique, les grandes bandes aurifères des Alleghanys et du Brésil, la zone stannifère de la Malaisie.)

Le même fait est bien connu pour les volcans; s'il en est d'isolés, la plupart constituent des séries; comme M. de Buch l'a depuis longtemps signalé quand il les compare à des soupiraux ayant pris naissance sur une même grande faille. Quant aux tremblements de terre, nous ne les mentionnerons que pour les rattacher aux volcans, auxquels ils semblent si intimement liés.

Les familles de sources thermales, de filons métallifères, de volcans, avec leurs tremblements de terre, occupent des étendues tout à fait comparables à celles que nous avons reconnues en métamorphisme régional, et dont le siège occupe des contrées entières.

De même que toutes ces familles, les terrains métamorphiques (au moins ceux qui sont postérieurs au terrain silurien) sont confinés exclusivement, comme on l'a dejà fait remarquer, dans les régions disloquées. . . . ." "Il est donc difficile de ne pas voir, dans les diverses espèces de phénomènes dont je viens de parler, les manifestations d'un même agent, dont le siège s'étend sous des pays entiers. Cet agent essentiel, c'est l'eau aidée de la chaleur à divers degrés, et

à laquelle se joignent, comme causes secondaires, les émanations qui l'accompagnent.

Pour les volcans la chose est évidente; pour les filons métallifères il ne peut plus guère exister de doute, surtout après le travail de M. Elie de Beaumont et les expériences de M. de Sénarmont, et, pour ce qui est du métamorphisme, nous croyons notre assertion devenue extrêmement probable.

Ainsi nous pensons que l'eau agit sans cesse dans les régions profondes, après y avoir acquis des températures plus ou moins élevées sous l'influence de la chaleur du globe." (*Op. cit.*)

With more special reference to metalliferous deposits in their relation with metamorphism, Daubrée remarks :—

" Les combinaisons métalliques provenant des profondeurs se sont très-fréquemment accumulées dans les fentes que présentaient les terrains et ont formé les filons métallifères.

Quelquefois aussi ces combinaisons et les divers autres composés qui les accompagnaient se sont répandus dans les roches, dont elles ont pénétré la substance en lui faisant subir les transformations profondes. C'est ainsi que s'y sont introduits, à proximité de roches éruptives, les amas de fer oligiste de Framont, les dépôts du Banat, des environs de Christiania et de Turginsk, où des minerais métalliques sont enchevêtrés au milieu de silicates, produits en même temps qu' eux dans la roche sédimentaire elle-même. De même, l'étain est arrivé dans beaucoup des amas qu'il constitue aujourd'hui, en réagissant profondément sur les roches encaissantes, comme je l'ai montré depuis longtemps, et en y engendrant des minéraux caractéristiques.

Quand des massifs entiers de terrains ont métamorphisé, il arrive parfois que, sur de vastes étendues, des substances métalliques se sont logées entre leurs feuillets, dans des conditions telles qu'il est impossible de résister à l'idée que leur arrivée est liée à la cause même qui a produit le métamorphisme. Comme exemple, je citerai l'or associé à la pyrite de fer ou au mispickel, dans le Zillerthal, en Tyrol; dans la Galice, en Espagne, où il est en outre accompagné d'étain; mais c'est surtout dans l'Oural, au Brésil, dans les Alleghanys, que ces terrains à la fois aurifères et métamorphiques atteignent de grandes dimensions.

En résumé, les dépôts métallifères, ainsi que les épanchements siliceux qui sillonent beaucoup de contrées, ne sont que des cas particuliers des phénomènes métamorphiques."

We see, therefore, that Daubrée made three important contributions to the theory of ore genesis, namely, (1) he emphasized the importance of fluorine as a mineralizing agent; (2) he gave reasons for believing that the water,

which, in his opinion, was so important an agent in regional metamorphism and ore genesis was in the main of atmospheric origin; and (3) he regarded the genesis of mineral veins as essentially a special incident in the hydrothermal activity involved in metamorphism.

T. BELT

The papers of Scrope, Elie de Beaumont, Scheerer and Daubrée, coupled with the confirmatory observations of Sorby, and the support given to them by authorities of the standing of B. von Cotta and Sir Charles Lyell, had by the middle of the 19th century secured strong recognition for the hydrothermal theory of the origin of metalliferous veins, which had by that time, among up-to-date students of the subject, effectively supplanted the earlier ore-magma and sublimation theories. Fournet, as we have seen, had ploughed a very lonely furrow as an advocate of the ore-magma hypothesis.

It is therefore of much interest to note that, at about that time, at least one very original English worker, Thomas Belt, was making observations in Australia and South America that led him to publish some very cogent arguments in favour of the ore-magma hypothesis. Belt was a first-rate and all-round naturalist as well as a highly-skilled mining engineer. He had made extensive observations among the gold mines in Victoria (Australia) before he took up gold mining in Nicaragua, and throughout his long experience he was a strong and consistent advocate of the view that auriferous quartz veins had been injected into the encasing rocks as ore magmas.

In a book entitled *Mineral Veins: an enquiry into their origin, founded on a study of the auriferous quartz veins of Australia,* published in 1861, Belt gave an account of the main features of the occurrence and distribution of auriferous veins in Victoria, and remarks as follows:—

"It struck me, early in my investigations, that the generally adopted hypothesis of the filling of mineral veins by sublimation, first started by Necker, was not applicable to the quartz veins of Victoria.

I found them filled with a homogeneous mass of anhydrous quartz, bearing no evidence whatever of gradual deposition on the sides of the fissures. I could not believe that silica, gold, iron, copper, lead, arsenic, sulphur and other minerals could be deposited from solutions without giving rise to a single instance of ribboned structure. In every vein I found proof of the contraction of the quartz after consolidation, and also that the contraction had not taken place until after the fissures had been entirely filled. Often the nature of the including rock rendered it essential that the fissures should be filled immediately after their formation; for how could they possibly remain open sufficiently long for the gradual deposition of several feet of quartz, when passing through strata so soft that the miner has to timber every foot of his shafts as he descends? And what, if such were possible, has obliterated all signs of such deposition?

The veins I examined appeared to be in their unaltered, normal condition, and I was forced back upon the old and now nearly discarded theory of their having been filled with molten quartz. I know well the difficulty of reviving the igneous theory of the origin of mineral veins, especially now when even the igneous origin of granite is questioned by able mineralogists and chemists. But its solution of the phenomena has appeared to be so conclusive, that I fully believe its adoption only depends upon the perspicuity with which I can lay my evidence and arguments before the scientific world."

In view, however, of the failure that had attended Fournet's long continued efforts towards a perspicuous statement of the evidence in favour of the ore-magma hypothesis, it was hardly likely that Belt would be any more successful; and little or no attention was paid to his views by his contemporaries. Indeed, it would be difficult to mention a publication that has received less notice, by reference and otherwise, among writers and workers on ore deposits than has this book by Belt on *Mineral Veins;* for until Belt's views were re-introduced at a very much later date by J. E. Spurr to account for the origin of auriferous quartz veins in the United States, they appear to have been almost completely ignored, and Spurr himself was at first quite unaware of them.

In the second chapter of his *Mineral Veins,* in which he deals with the connection between mineral veins and intrusive rocks, Belt mentions with approval the observations already quoted from Darwin with reference to the intimate

association of metalliferous veins with the intrusive and metamorphic rocks of central Chile, and remarks that this association is to be observed not only in South America, but throughout the world.

Having shown, to his own abounding satisfaction, that "the production of fissures, and their injection with fused matter, are the natural results of plutonic action, and that the igneous theory is sufficient to account for the primary forms and the arrangement of minerals in lodes," he summarizes his conclusions as regards the origin of mineral veins as follows :—

"1. The auriferous quartz veins of Australia are filled with minerals which are not liable to be decomposed by the action of water, and which apparently now exist in the same state as that in which they were originally deposited.

2. In these veins, the distribution of the gold, and the structure and arrangement of the quartz, are explained by the theory that they are fissures that have been filled with molten silica containing entangled metallic vapours.

3. Mineral veins are constantly found in connection with igneous rocks, and in some cases, as in Cornwall and Wicklow, a regular sequence of events have followed the intrusion of molten granite, by which granitic, porphyritic and mineral veins have been successively formed.

4. The fusion of rocks in the bowels of the earth, and their subsequent consolidation, supply the requisite conditions for the rending open of the superincumbent rocks, and the filling of the vents so formed with fluid matter, varying in composition according to the comparative depth from which it has been projected.

5. The objections raised against the igneous theory of quartz veins and of granite are not tenable, being based either on a misapprehension of the theory, on a misinterpretation of observed facts, on experiments where the natural conditions were not fulfilled, or on the obscurity in which certain delicate chemical questions are still involved.

6. The investigation of the origin of lodes of the baser metals in Europe has been impeded by the confusion arising from the mixing up of the results due to secondary agencies with those referable to original deposition.

7. Mineral veins and trappean dikes have many features in common, and the points in which they vary may be explained by a reference to the different conditions under which the igneous matter has been developed."

At a later date appeared Belt's more widely read book *The Naturalist in Nicaragua* (1874), which was highly appreciated by Charles Darwin, and which is still sufficiently popular to be reprinted for 20th century readers. In a chapter of this book, Belt returns to the subject of mineral veins, and reiterates, with some additional features, the views expressed in the earlier book. Apparently anxious not to be regarded as too singular in his views as to the igneous origin of auriferous quartz, he refers to papers by David Forbes, who had pointed out that auriferous veins were associated with two different types of intrusive rock, viz., granitic and dioritic, and that this igneous association with gold is not confined to South America, but appears to prevail in other parts of the world (Quart. Journ. Geol. Soc., vol. 17; and Geol. Mag., Sept., 1866). He also refers to a paper entitled *Notes on the Geology of Queensland* by R. Daintree (Quart. Journ. Geol. Soc., vol. 28) in which the author shows that auriferous veinstones are there associated with certain intrusive trap rocks, and that even some of the trappean dikes themselves are auriferous. He (Belt) then remarks (*Op. cit.*, p. 96):—

"I myself, several years ago, endeavoured to show that mineral veins in granitic districts occurred in regular sequences with certain intrusive rocks, as follows:—1st, intrusion of main mass of granite; 2nd, granitic veins; 3rd, elvan dikes; and lastly, mineral veins cutting all other intrusive rocks.

In every region of intrusive plutonic rocks that has been thoroughly explored, a similar succession of events, culminating in the production of mineral veins, has been proved to have taken place, and it appears that the origin of such veins is the natural result of plutonic intrusion. There is, also, sometimes a complete gradation from veins of perfectly crystallized granite, through others abounding in quartz at the expense of other constituents, up to veins filled with pure quartz. . . . Granitic, porphyritic and trappean dikes also sometimes contain gold and other metals; and I think the probability is great that if dikes and veins of granite have been filled by igneous injection, so have those of quartz. By an igneous injection, I do not mean that the fused rock owed its fluidity to dry heat. The celebrated researches of Sorby on the microscopical fluid cavities in the quartz of granite and quartz veins have shown beyond a doubt that the vapour of water was present in comparatively large quantities when quartz was solidifying. All strata

below the surface contain water, and if melted up would still hold it as superheated steam; and M. Angelot has suggested that fused rock under great pressure may dissolve large quantities of the vapour of water, just as liquids dissolve gases. The presence of the vapour of water would cause the liquefaction of quartz at a much lower temperature than would be possible by heat alone, unaided by water. I know that this opinion is contrary to that usually held by geologists, the theory generally accepted being that mineral veins have been produced by deposits from hot springs; but during twenty years I have been engaged in auriferous quartz-mining in various parts of the world, and nowhere have I met with lodes the phenomena of which could be explained on this hypothesis."

As regards the geodynamic factors involved in ore genesis, Belt in this book expanded somewhat on the views he had expressed in *Mineral Veins,* and emphasized the factor of metamorphism, as follows:—

" Sedimentary strata have been carried down by movements of the earth's crust, far below the surface, covered with other strata, and subjected to great heat, which, aided by the water contained in the rocks and various chemical reactions, has effected a re-arrangement of the mineral contents of the strata, so that by molecular movements the metamorphic crystalline rocks, including interstratified granites and greenstones, have been formed.

Carried to greater depths and subjected to more intense heat, the strata have been completely fused, and the liquid or pasty mass, including the contorted strata above it, has formed perfectly crystalline intrusive granites and greenstones."

The cracks and fissures developed as a consequence of this geodynamic process were filled by injections of molten rock or ore magma, and the metalliferous veins thus formed have, according to Belt, for the most part retained the primary characteristics they acquired at the time of injection. He allows that, during the long period of time that has elapsed since their formation by injection, the vein fissures have been re-opened in many cases, and that minerals such as fluorspar, quartz and various metallic ores have been deposited in them by hydrothermal or aqueous action subsequently to the original formation of the lodes; but he claims that:—

" in auriferous quartz lodes both the metal and the veinstone have generally resisted all these secondary agencies, and are presented to

us much the same as they were first deposited, excepting that the associated minerals have been altered and in some cases new ones introduced, by the passage of hot springs from below or percolation of water from the surface."

From a comparison of these statements by Belt with those already quoted from Fournet, the reader will see that there is very close agreement between the views of the two authors as regards the origin of metalliferous veins, as also there is between the views of both these authors and those advanced in recent years by J. E. Spurr.

T. STERRY HUNT

One of the most active investigators and stimulating writers on geology during the third quarter of the nineteenth century was T. Sterry Hunt, a forceful thinker and diligent student of ore genesis, whose collected papers entitled *Chemical and Geological Essays* (1878) contain much that is still worth reading by students of geochemistry. He held much the same views as Bischof, and considered it unnecessary to regard granites as congealed molten silicates. On the contrary he regarded them as low-temperature alteration products of sediments. Thus, in a paper entitled *The Geognostic History of the Metals* (Amer. Inst. Min. Eng., 1873) he remarked:—

"If the view which I hold, in common with many other geologists, that most, if not all, of our known eruptive rocks are but displaced and altered sediments, be true, then it may be fairly affirmed, not that eruptive rocks are the agents which impregnate sedimentary deposits with metals, but on the contrary, that in such deposits is to be sought the origin of metalliferous eruptive rocks, and that all our metallic ores are thus to be traced to aqueous solutions."

He favoured Scheerer's view that

"congealing granitic rocks had been impregnated with a juice, which was nothing else than a highly-heated aqueous solution of certain mineral matters. This, under great pressure, oozed out, penetrating even the stratified rocks in contact with the granite, filling cavities and fissures in the latter."

He strongly opposed the view advocated by Fournet, that granites had been in the condition of anhydrous masses of molten magma.

While claiming, however, that granite intrusions and their closely allied vein rocks were originally aqueous solutions of mineral matter, he did not regard these solutions as of much importance in connection with the direct origin of metalliferous veins. On the contrary, he held that fissures of the type filled with zinc-lead ores, calcite and barite, such as those of the Carboniferous Limestone in England, were open to the surface at the time they were filled, and that they were infilled by solutions of superficial origin. In the *American Journal of Science* for May, 1861, he sets forth his views on this matter as follows:—

"The metals . . . seem to have been originally brought to the surface in watery solutions, from which we conceive them to have been separated by the reducing agency of organic matters in the form of sulphurets or in the native state, and mingled with the contemporaneous sediments, where they occur in beds, in disseminated grains forming fahlbands, or are the cementing material of conglomerates. During the subsequent metamorphism of the strata, these metallic matters, being taken into solution by alkaline carbonates or sulphurets, have been redeposited in fissures in the metalliferous strata, forming veins, or, ascending to higher beds, have given rise to metalliferous veins in strata not themselves metalliferous. Such we conceive to be, in a few words, the theory of metallic deposits; they belong to a period when the primal sediments were yet impregnated with metallic compounds which were soluble in the permeating waters.

. . . .

The intervention of intense heat, sublimation and similar hypotheses to explain the origin of metallic ores, we conceive to be uncalled for. The solvent powers of solutions of alkaline carbonates, chlorides and sulphurets at elevated temperatures, taken in connection with the notions above enunciated, and with De Senarmont's and Daubrée's beautiful experiments on the crystallization of certain mineral species in the moist way, will suffice to form the basis of a satisfactory theory of metallic deposits."

In this essay on *The origin of metalliferous deposits* (*Op. cit.*), Sterry Hunt pointed out that the process of ore deposition was one involving the segregation of formerly diffused metals, since the elements must originally have been diffused through the earth's crust. He asked:—

"How have these elements thus been brought together, and why is it that they are not all still widely and universally diffused? Why

are compounds of iron in beds by themselves, copper, silver and gold gathered together in veins, and iodine concentrated in a few ores and certain mineral waters?"

He answered these questions by saying that water is a universal solvent, and that, aided in some cases by heat, pressure, and the presence of certain widely distributed substances, such as carbon dioxide and alkaline carbonates and sulphides, it was capable of dissolving the least soluble bodies. He was also much impressed with the significance of organic agencies in ore deposition, and went so far as to say:—

"I can hardly conceive of an accumulation of iron, copper, lead or gold, in the production of which animal or vegetable life has not, either directly or indirectly, been necessary."

He pointed out that pyrite, like iron oxide, was forming at the present time in certain waters and in beds of mud and silt where it sometimes took on good crystalline development, and indicated the importance of the action of organic matter in this connection. Further, marine plants absorb iodine, potash, copper, silver and lead from sea water, and copper is present in the blood of many marine mollusca. Marine organisms that have absorbed metals, accumulate and decay in lagoons and on the sea floor, generating the sulphides necessary to fix the metals in an insoluble form in the sediments, thus removing them from the terrestrial circulation.

Metalliferous waters circulating at the surface may also become concentrated in enclosed basins. He quotes a case of an English copper mine, the waters of which impregnated the turf of a neighbouring bog to such an extent that its ashes were found to be a profitable source of copper.

Hence, when sediments become buried by superincumbent detritus, they may contain appreciable quantities of metalliferous material. This is at first disseminated through a large mass of sediment, but it is ultimately dissolved by circulating solutions, which find their way into fissures in the adjacent rocks. Such, according to Sterry Hunt, are

the beneficial results of vein-making by what he calls Nature's process of concentration.

Sterry Hunt's views had much influence in shaping the notions on ore genesis that prevailed in North America during the latter half of the nineteenth century. They were essentially the same as those advocated by S. F. Emmons at the date of the famous paper by Pošepný, and by Van Hise even at a much later date. Unfortunately for the broadly sound scientific views of Emmons and Van Hise, there was in their days, as indeed there has been since, much confusion of thought as between the narrow and inadequate lateral secretion hypothesis of Sandberger and the broader and more soundly geological views of Emmons and Van Hise. For want of a better name these views have been usually referred to as the theory of lateral secretion; but it is rather absurd to confuse a large and comprehensive geological theory of ore deposits such as that advocated by Sterry Hunt, Emmons and Van Hise with the much narrower conception of lateral secretion entertained by Sandberger.

While Sterry Hunt was not alone in his day in advocating these views, much credit is due to him for his very clear and terse statement of, and insistence on, the important part played by exogenetic processes in the origin and segregation of metalliferous deposits. Of special interest is his emphasis on the part played by organisms and organic matter, a view of things which has been emphasized afresh in recent years by Samoilov and others (see particularly *Palæophysiology: the organic origin of some minerals occurring in sedimentary rocks,* by J. V. Samoilov; Mineralog. Mag., vol. 18, 1917).

W. WALLACE

It will be noted that Sterry Hunt's advocacy on behalf of low-temperature solution effects in ore genesis was carried on at about the same time as Belt was writing in support of the ore-magma theory. Sterry Hunt's paper on the *Origin of Metalliferous Deposits* was published in the same year (1861) as Belt's *Mineral Veins.*

It is therefore interesting to note further that, also in the same year (1861), there appeared in England an interesting work by William Wallace entitled *Mineral Deposits: The Laws which regulate the deposition of Lead Ore in Veins,* in which he gave an account of the geological structure of the mining district of Alston Moor in the north of England, and explained the formation of the veins as due to the action of descending solutions of atmospheric origin. The hypothesis that mineral veins owe their formation to solutions of igneous origin ascending from the interior of the earth, he regarded as having no adequate basis in fact; and he emphasized the significance of the circulation of meteoric waters in the fissures of emergent land masses exposed to the action of atmospheric precipitation.

"In Alston Moor, the veins have been most productive in situations farthest removed from plutonic action. The richest deposits have been formed in the upper part of the Mountain Limestone, where no igneous rocks are found, either in the form of dykes, or sheets intermingled horizontally with the stratified rocks. . . . So far as this district is concerned, there is nothing to support the theory that lead is due to exhalations from beneath or to matter injected in a fluid state among the consolidated sedimentary rocks" (*Op. cit.*, p. 99).

He pointed out that nodules of carbonate of iron, found in the shales, showed shrinkage cracks and cavities in which sulphides of lead, zinc, iron, copper and various other minerals had not infrequently been formed. These, he claimed, could not have been formed by exhalations from below, but were rather due to infiltration from above.

For the formation of the veins he had studied in the Alston Moor mining district, Wallace thought the essential factors were the passage of large bodies of meteoric water downward from the surface of exposed and elevated land masses, the circulation of these waters at moderate depths in fissures and at some distance from the watershed of the mountains, and electrical action in the fissures. According to him the operation of vein formation is still in progress under the influence of these agencies.

A few years later, in a *Report on Mineral Veins in Carboniferous Limestone and their organic contents* (Rept. Brit. Assoc., 1869, p. 360), Charles Moore gave an account of the palæontology of mineral veins, with long lists of the fossils occurring in them, and attributed the formation of the veins to deposition from ocean waters. He claimed that the elements represented by vein minerals were present in small quantities in ocean waters, which gained access to fissures during a marine transgression, and that it was to such transgressions that mineral veins owed their formation, and not to the agency of atmospheric waters on exposed land surfaces as suggested by Wallace.

Thus, for the formation of mineral veins, according to Moore, it is necessary for ocean waters to have access to open fissures. For the rest, only favourable electrical conditions and time for precipitation are necessary to complete their formation.

CHAPTER X

NINETEENTH CENTURY (FOURTH QUARTER)

VON GRODDECK, EMMONS, PHILLIPS, SANDBERGER, STELZNER, POŠEPNÝ, LE CONTE, VOGT AND DE LAUNAY

THE last quarter of the nineteenth century was a period of active investigation and controversy on the subject of the genesis of ore deposits. At the outset of this period, perhaps the dominant viewpoint in Europe was that represented by Von Cotta, whose *Die Lehre von den Erzlagerstätten* continued to be popular, as indicated by the fact that the 2nd edition of this work, the 1st edition of which had appeared in 1859, was translated into English as late as 1879.

A. VON GRODDECK

In that year, another *Die Lehre von den Lagerstätten der Erze,* by A. Von Groddeck, was published in Germany. This, like Von Cotta's textbook, was an excellent treatise on the subject, and did full justice to the various possibilities as regards ore genesis; but while tending on the whole to maintain the Von Cotta tradition by paying respect to all the various theories that had a sound geological basis, he was perhaps somewhat less inclined than Von Cotta to give special favour to the ascending hot water infiltration theory. Having reviewed the various theories then in vogue, including those of lateral secretion, magmatic injection, sublimation, and infiltration by ascending hot waters, he remarked :—

" So sehen wir also, das eine Erklärung nicht für alle Gänge passt, und dass die verschiedenen Forscher, welche die angeführten Theorieen aufstellten, nur darin fehlten, dass sie, aus einzelnen Fällen, richtig abgeleitete Anschauungen unlogisch verallgemeinerten."

S. F. EMMONS

In the United States, where during this last quarter of the nineteenth century there sprung up a very active interest in ore genesis, the dominant views at the outset of this period were those of Sterry Hunt, who, as we have already seen, regarded metalliferous veins as due to deposition from waters of surface origin, either through the action of strictly lateral secretion, or through the action of water that had circulated more extensively in the earth's crust, depositing their metalliferous matter in fissures or other cavities, or by replacement of the rocks they traversed. This was the view that was advocated by S. F. Emmons and others about that time. It was the view expressed in his U.S. Geol. Surv. Monograph on the ore deposits of Leadville, Colorado, in 1886 by Emmons, who regarded these deposits as due to replacement of country rock by descending solutions which had leached their metalliferous matter from the neighbouring eruptive rocks (porphyries). This view proved to be unpopular when it was attacked by Pošepný in 1893, after which date the ascending hot water infiltration theory became dominant in the United States.

As will be seen below, however, Pošepný advocated a comparatively irrational view as to the origin of the metals, namely, that they were derived from the barysphere. This view, which he held in common with de Launay, was no doubt prompted by the very sound consideration that the igneous rocks of the silicate crust, as we know them, do not contain sufficient lode-forming metalliferous material to furnish the infillings of metalliferous veins. In his paper on *The Genesis of Certain Ore-Deposits* (Trans. Amer. Inst. Min. Eng., 1886-87, vol. 15, p. 125), Emmons claimed that ore deposition was effected by water of meteoric origin, and that the generating solutions derive their load of metalliferous matter from neighbouring rock masses in the earth's outer crust, and not from the barysphere. These claims by Emmons are far more in accordance with the facts of dynamical geology than are the claims put forth by the

baryspheric school represented by de Launay, Pošepný and others.

In France, Daubrée, as we have already seen, also held the view that metalliferous deposits had been formed by the action of waters of superficial origin, and that ascending hot waters were merely such superficial waters which had been able to penetrate sufficiently far into the earth's crust to become heated and resurgent by contact with hot rocks. In France, however, as in the United States, this view did not secure a good footing, and was soon to become overgrown by the revival of the magmatic differentiation theory, chiefly owing to the influence of L. de Launay.

J. A. PHILLIPS

In England, where comparatively very little interest had been shown in this subject since the opening years of the century, Prime's translation of Von Cotta's *Lehre,* the first edition of which had appeared in 1859, was, as already mentioned, published as late as 1879. It should be mentioned, however, that J. A. Phillips had been for some time engaged in active investigation of British ore deposits at the outset of the period under consideration. His paper on *The Rocks of the Mining District of Cornwall and their relation to Metalliferous Deposits* (Quart. Journ. Geol. Soc., vol. 31, 1875) was, as regards the origin of metals, inclined to the same theory as that adopted by Daubrée in France, and Sterry Hunt and S. F. Emmons in the United States, as shown by the following passages quoted from the paper referred to:—

"The vein fissures of the tin- and copper-bearing lodes of Cornwall were generally the result of forces acting subsequently to the solidification of the elvans, but operating in the same general direction as those which caused those rocks to be erupted.

The fissures thus produced afterwards became filled with minerals resulting from deposits by chemical action, from waters and aqueous vapours circulating through them. From coming into contact with highly heated rocks at great depths, these waters were sometimes at a high temperature; but it is probable that, in many cases, the heat was very moderate, and the action comparatively slow.

To what extent these deposits were produced by waters rising from below, and how far they were influenced by lateral percolation cannot be determined. The effects produced on the contents of veins by the nature of the enclosing rock, and the frequent occurrence of deposits of ore parallel with the line of dip of the adjoining country, would, however, lead to the conclusion that lateral infiltrations must have materially influenced the results.

Contact deposits and stockworks have been formed by analogous chemical actions, set up in the first case in fissures resulting from the junction of dissimilar rocks, and in the second, in fractures produced during the upheaval of partially consolidated eruptive masses. The alteration experienced by stratified deposits in the vicinity of eruptive rocks is probably often due to somewhat similar percolation."

In his *Treatise on Ore Deposits* (1884) Phillips declared in favour of lateral secretion in the restricted sense. He thought it unlikely that the flow of surface waters into vein fissures, as suggested by Wallace, had, as a rule, materially added to the richness of mineral veins.

"It appears to be now well established that the heavy metals occurring in metalliferous veins in the form of ores are, in the state of silicates or of other mineralized combinations, present in greater or less proportions in rocks of almost every age, and that these are capable of supplying all the chemical constituents of the different ores and veinstones of the lodes passing through them.

That the minerals thus disseminated throughout the rocks have been originally the source of the metalliferous accumulations which have taken place in veins admits of little doubt, but we have still much to learn with regard to the process by which ores of the different metals have become concentrated in fissures. In the majority of cases this must have been effected by chemical solution and subsequent deposition, as indicated by the comby structure of many lodes, as well as by the occurrence one upon another, of such minerals as quartz, calcite and ores of iron. . . .

There is reason to believe that lodes may have often been produced by lateral secretion at ordinary temperatures, and that the ores and other minerals constituting veins may have been deposited in approximate vicinity to the points at which the solutions entered the fissure. As, however, the fissures of true veins are supposed to extend far into the earth, we are justified in believing that the solvent powers of the menstrua, acting upon minerals disseminated through the strata, will be increased by a high temperature and the pressure incident to great depth. These heated waters, obeying known laws, will have a tendency to ascend, and in doing so will gradually lose their power of holding minerals in solution, and a deposit on the surfaces of the fissures will

be the result. Metalliferous veins are of more frequent occurrence in the neighbourhood of eruptive rocks than in other situations and it is probable that these may have not only been instrumental in producing fissures, but may have also contributed to supply heat to the waters circulating through them."

In the second edition of the *Treatise on Ore Deposits* (1896), revised by Henry Louis, attention is given to the American revival of the igneous (ascensionist) theory by Pošepný and others. Louis gives the reader an excellent discussion of the problem, and points out

" that the mode of formation of metalliferous veins is very far indeed from being understood. . . . The manner in which the minerals were deposited in the fissures admits of various interpretations. Even the statements upon which the opposing theorists base their arguments are not by any means unchallenged by their opponents, and even if they were, most of them admit of diametrically opposite explanations. Thus, if it were definitely proved that the rocks within reasonable distance of a fissure vein contain minute proportions of the same metals as found in the vein, one side would see in this fact a proof that the metals in question were derived from the rocks, whilst the others would argue that the metals in the rocks were derived from the vein. . . . In spite of all that has been done and written on this subject, we are still reduced to mere conjecture on two of the most important elements of the inquiry, whence the metals were dissolved and how they were precipitated. It is only safe to affirm that they were in very many cases introduced in the form of solutions."

Since the time, now nearly forty years ago, when Louis wrote those words, opinion has grown strongly and almost unanimously in favour of the igneous theory; but all the same, so far as concerns the actual facts of the problem, the statement by Louis as quoted above could scarcely be regarded as inapt if applied to the position at the present time.

We see, therefore, that at the outset of the last quarter of the 19th century, much support was given by various influential investigators to the view that metalliferous vein deposits owed their formation to the leaching action of waters of surface origin on the rocks of the earth's crust. We shall also see, however, that although this view was shortly to receive strong support for a time by the results of the work of Sandberger, it was soon afterwards to suffer comparative

eclipse as a result more especially of the work of Pošepný, Suess, de Launay and others, who favoured the view that the metalliferous solutions from which vein deposits had been derived owed their direct or immediate origin to exudation from deep-seated molten magmas, and not to infiltration from the surface. E. Suess's view (see Verh. Gesell. deutsch. Naturf. 1902) that the water of hot springs such as those of Karlsbad are "juvenile," i.e., that they are exudations from igneous magmas reaching the surface for the first time, has attained wide popularity among geologists and students of ore genesis in opposition to Daubrée's view that such water is of superficial origin.

F. SANDBERGER

The theory of lateral secretion, which was among the earliest advanced to account for the origin of mineral veins, and the applicability of which had been recognized by Bischof, was advocated in an extreme form in his *Untersuchungen über Erzgänge* (vol. 1, 1882; vol. 2, 1885) by F. Sandberger, who sought to establish this theory on a firm basis of fact as regards the relative composition of veins and the country rocks they traversed. For this purpose he carried out numerous quantitative analyses on country rocks and veins in the Schwarzwald and elsewhere, and found that the materials of the veins were usually disseminated in the country rocks. Similar results, showing the wide distribution of small quantities of base metals and other metals in ordinary rocks, had already been obtained by G. Forchammer (Pogg. Ann., vol. 95), A. Daubrée (Compt. Rend., 1851, vol. 32) and were later confirmed by many other workers.

Quite apart from their bearing on the theory which Sandberger sought to establish by this work, the results he obtained were very valuable as showing the widespread distribution of metals in rocks and their association with various minerals in igneous rocks. He showed that olivine usually contained nickel, copper and cobalt. In addition to these metals, he found lead and zinc in the augites of gabbros,

dolerites and basalts, while in some instances, antimony and arsenic were also present. Lithia mica he found to contain tin, arsenic, copper, bismuth and sometimes uranium. In the biotite mica of Spessart he found copper, cobalt, nickel and bismuth, while in biotite from Schemnitz he found arsenic, lead and zinc. He also found considerable amounts of fluorine in micas, and baryta in felspars. It had already been shown that copper, lead and zinc existed in appreciable quantities in slates, and Bischof had contended that metals were widely distributed in other sedimentary and metamorphic rocks.

It was upon these facts concerning the widespread distribution of metals in rocks, and the relation of metalliferous veins thereto, that Sandberger based his view that the veins owed their origin to accumulation in fissures by lateral secretion from the immediately neighbouring rock, rather than by migration in ascending or descending solutions to the place of deposition. The engaging simplicity of this view of things caused it to make a strong appeal to many workers, but it was not widely held, in the strict sense in which Sandberger held it, by the more profound students of ore genesis, most of whom, while admitting the possibility that in many cases lateral secretion of the metals had occurred, regarded vein formation as a much more complicated process, in which migrant solutions that had travelled a considerable distance had probably in many cases played an important part.

While, however, it may be admitted that Sandberger was too much of an extremist in his advocacy of the lateral secretion theory, it should not be forgotten that his *Untersuchungen über Erzgänge* was an important piece of work, more especially as proving conclusively the widespread distribution of metals in rocks, and their characteristic mineral associations.

A. W. STELZNER

Sandberger's paper roused considerable interest, and was well received by numerous workers who favoured the

view that comparatively low-temperature solutions of surface origin had been responsible for ore deposition. Before long, however, it was opposed by A. W. Stelzner, who was a strong supporter of the view that the metalliferous solutions responsible for ore deposition had been derived from deep-seated sources. His contentions against the lateral secretion theory in Zeits. deutsch. geol. Gesell. (vol. 31, 1879), and in his later treatises on ore deposits, had much effect in checking support for this theory in its restricted sense.

Pošepný and other workers also challenged the views of Sandberger, and it was claimed that, instead of the veins being derived by leaching from the country rock as Sandberger supposed, the country rock had in some cases clearly derived its metalliferous matter from the veins.

This opposition to Sandberger's lateral secretion theory by Stelzner and Pošepný, and the failure of efforts to confirm it among mining engineers who tested it in various mining districts, prevented the theory from enjoying for long the popularity which it attained for a time by virtue of its attractive simplicity. As already mentioned, however, the more comprehensive theory that had been advocated by Daubrée, Sterry Hunt and others, continued to receive support by S. F. Emmons, Van Hise and others, and indeed still receives some support at the present time.

POŠEPNÝ

The paper on *The Genesis of Ore Deposits* (Trans. Amer. Inst. Min. Eng., 1894 for 1893, vol. 23), by F. Pošepný, was an important contribution to the study of this problem, not merely because it was a well-wrought statement of Pošepný's views, embodying much of his field experience as an investigator, but also because the stage was well set for its reception, and opinion in the United States was ready to react strongly against such views as those of Sandberger and S. F. Emmons. Other views, however, were seeking for favour; for while Sandberger had been working to establish the theory of lateral secretion, Vogt and others were

emphasizing the importance of magmatic segregation. Pošepný was of opinion that neither lateral secretion nor magmatic segregation were of any considerable significance as processes in ore deposition, although he did not deny that they were of limited significance.

As regards magmatic segregation, he remarked:—

"With respect to ferriferous oxides, this view has some foundation; but the notion, apparently held in some quarters, that sulphides also were thus segregated from the magma, surpasses my comprehension. It is true that pyrite is sometimes seen upon the lavas of active volcanoes; but this occurs, so far as I know, only when fumaroles or solfataras emit gases and vapours which decompose the rock, and therefore the agency of the solvent is not lacking. I am therefore obliged to conclude that aqueous solvents are the chief factor in the genesis of ore deposits, and I shall be guided by this principle."

As regards the lateral secretion theory, he based his opposition to this on work he had done in the Przibram district of central Bohemia, the results of which he summed up as follows:—

"This district was made a test of Sandberger's lateral secretion theory. Careful and repeated analysis showed the presence of metals in the rocks, but could not decide the question whether the metals were primitive ingredients or secondary impregnations. Since such metallic traces occur in both the eruptive and sedimentary rocks, but cannot possibly be in both cases primitive, it is probable that they are in both cases secondary. There is then, in this case, notwithstanding the connection of ore veins with the dikes, no proof that they were formed by the leaching of the country rock. If the vein material (as is very likely) was derived from eruptive rocks, these were situated much deeper than the eruptive rock disclosed down to 3,600 ft. below the surface, or 1,850 ft. below sea-level."

"The Cambrian sandstone basin of Przibram is unsymmetrical; one side dips gently north-west, the other (next to the fault) slightly south-east. In the latter part, which is also more highly metamorphosed, lies the bonanza or rich ore ground, which therefore starts from the intersection of the great structural fault with the zone of eruptive rocks, in other words, from the point relatively nearest to the barysphere."

From these considerations Pošepný was led to infer that there are two definite and distinct regions of aqueous circulation in the earth's crust; one the outer or vadose region in which ore deposition took place from descending waters

or by lateral secretion, the other and more important one being the deeper crust in which deposition was due to hot waters ascending from the barysphere. He held the view that eruptive rocks brought with them metalliferous substances from the barysphere into the lithosphere, but that ascending hot waters were the agency by which the metals were deposited in the outer crust, and claimed in this respect to be in agreement with de Launay.

Pošepný's paper roused much discussion, most of which was highly favourable to his views, especially among the mining engineers, most of whom seemed to welcome a return to the ascending juvenile water theory, the merits of which had been so recently challenged by Sandberger. The geologists, however, as represented by Le Conte, who made a very excellent contribution to the discussion, were strongly opposed to the notion that metalliferous solutions or eruptives were of baryspheric origin.

LE CONTE

J. Le Conte, in a contribution to the voluminous discussion on Pošepný's paper (Trans. Amer. Inst. Min. Eng., 1895, vol. 24, p. 996), expressed the opinion that,

> "in his zeal against the extreme lateral-secretion views of Sandberger, Pošepný had gone to the other extreme of ascensionism; and that a truer view than either may be found in one that shall combine and reconcile the two extremes."

Le Conte denied that the barysphere was within reach of circulating water. He admitted the existence of constituent water in deep-seated igneous matter, which water had been occluded in primal magma; but he was of opinion that this water did not circulate. Osmond Fisher had already expressed the opinion that this water of deep-seated origin (later termed juvenile water by Suess) was still escaping, and that in doing so, it fused its way towards the surface, finally emerging in volcanic eruptives; but this view of things Le Conte regarded as very doubtful, believing that the water involved in volcanic eruptives was non-circulating meteoric water.

The only circulating water in the earth's crust the existence of which Le Conte would admit was meteoric water, i.e., water that had seeped into the crust from the earth's surface. He held the opinion that this circulating water extended to a depth of eight or ten miles from the surface, but not deeper, and that the metals of ore deposits had been derived from leachings within this depth. He pointed out that down to this limit, there was no " barysphere " in any intelligible sense of that word.

He continued as follows:—

" I believe, therefore, that the greater abundance of metallic ores in solution in ascending waters is the result, not of the greater abundance of metals in their lower courses, but of the greater heat which they take up in that part of their course, and the greater pressure to which they have been subjected there. Both heat and pressure greatly increase the solvent power of water upon the feebly soluble metallic sulphides. Thus heavily freighted, the waters lose, in ascending, both heat and pressure, and therefore deposit abundantly in their upward course. In a word, ascending waters are rich in metallic contents, not because they have traversed a barysphere, but because they have traversed a thermosphere. With equal heat and pressure, I am convinced they would get as much metal from our more basic rocks here at the surface, as they do now from the hypothetical barysphere. These ascending waters are non-oxidizing, not because they have never seen the air, but because they have exhausted their oxidizing power by previous oxidation of metals, of organic matters, and other oxidizable substances in the upper parts of their downward course."

Le Conte summarized his views on ore deposition as follows:—

" Ore deposits, using the term in its widest sense, may take place from many kinds of waters, but especially from alkaline solutions; for these are the natural solvents of metallic sulphides, and metallic sulphides are usually the original form of such deposits.

They may take place from waters at any temperature and pressure, but mainly from those at high temperature and under heavy pressure, because, on account of their great solvent power, such waters are heavily freighted with metals.

The depositing waters may be moving in any direction, ascending, horizontally moving or even sometimes descending, but mainly ascending, because by losing heat and pressure at every step, such waters are sure to deposit abundantly.

Deposits may take place in any kind of water-ways in open fissures, in incipient fissures, joints, cracks and even in porous sandstone, but especially in great open fissures, because these are the main highways of ascending waters from the greatest depths.

Deposits may be found in many regions and in many kinds of rocks, but mainly in mountain-regions and in metamorphic and igneous rocks, because the thermosphere is nearer the surface, and ready access thereto through great fissures is found mostly in these regions and in these rocks."

J. H. L. VOGT

Towards the end of the nineteenth century, J. H. L. Vogt gave an account of the formation of ore deposits by differentiation processes in basic eruptive magmas (Zeits. prakt. Geol., 1893, vol. 1, pp. 4, 125 & 257). In this paper, Vogt described igneous segregations of ilmenite and titaniferous iron ores, and segregations of nickeliferous pyrrhotite in basic igneous rocks such as norites and gabbros. Before that date he had attributed the formation of magnetite deposits in the Silurian rocks of the Christiania district to impregnation by iron-bearing vapours derived from neighbouring igneous rocks. These and later papers by Vogt, de Launay, Brögger and Weinschenk did much to secure recognition of the importance of magmatic differentiation as a process of ore deposition, although it should be mentioned that this process had been very clearly outlined previously by Elie de Beaumont and other early workers (see quotation already given from E. de Beaumont's paper).

Vogt and other Norwegian workers have further invoked the magmatic differentiation theory to account for the origin of pyritic deposits, such as those of Röros and Sulitjelma in Norway. These are regarded as having arisen by the segregation of a thin fluid sulphide magma in a larger magma of gabbroidic composition, and as having been subsequently injected at great depth and pressure into the slates and schists in which they occur.

This notion represents an extension of the conception of rock magmas. According to it, a rock magma is not necessarily a silicate magma, but may consist of some fairly simple non-silicate segregation or differentiation product such as

chromite, ilmenite, pyrrhotite, pyrite, quartz, etc., deposits of all of which may, according to this theory, arise by magmatic injection. It is thus a return in some measure to the old views of Hutton and Fournet, namely, that certain ore deposits, like ordinary igneous rocks, have arisen as molten magmas.

Although Vogt advocates magmatic injection as the best way of explaining the origin of certain ore deposits, such as those referred to above, he regards it as strictly limited in significance and does not extend it to all sulphides or other vein deposits. He has made his position in this respect quite clear in a contribution to the Engineering and Mining Journal, New York, 1927, vol. 123, p. 684, in which he declares his adherence to the view of the "old German School" that, as a rule,

> "veins are due to hydrothermal solutions, derived from an 'unknown depth,' within each vein fissure at the various stages of varying compositions and the filling continued during long geological periods."

Vogt thus declares himself to be, not an ore-magma extremist, but a supporter of both the "hot-water" and magmatic differentiation schools:—

> "In my opinion there is, however, no opposition between the 'hot-spring' school and the theory of magmatic differentiation. I am myself an adherent of both schools of theories. The old German 'hot-spring' school left the problem of the original source of the metals in the 'unknown depth' an open question. The magmatic differentiation school tries to advance further towards the solution of the problem. We have determined the formation of several classes of ore deposits by various processes of liquid magmatic differentiation. As to other classes of ore deposits we have determined a formation by an extraction of the metallic contents in the magma due to the dissolved volatile compounds. And as to still other classes of ore deposits, we may also conclude that the metallic contents have been derived from the original rock magmas, and probably as a rule from the residual magmas, but in what manner the magmatic extraction has taken place may be left to future investigations"

L. DE LAUNAY

The traditions of the French school of workers on ore genesis, carried on continuously through the nineteenth century from the days of D'Aubuisson de Voisin, by Fournet,

Burat, Elie de Beaumont and Daubrée, have been maintained in admirable style in recent years by L. de Launay, whose excellent treatises on geology and ore deposits, commencing with the *Formation des Gîtes Métallifères* in 1893, to be followed later by his *Traité de Métallogénie, La Science Géologique* and other works, have been widely read for many years.

As regards the nature and origin of waters circulating in the earth's crust, de Launay has on the whole followed Daubrée :—

"Les sources thermales représentent, en principe, la réapparition au jour d'eaux, infiltrées à la superficie, descendues en profondeur, où elles se sont échauffées et minéralisées et remontées rapidement vers la surface à la faveur de quelque grande fracture.

Il est possible qu'il y ait, en outre, dans les régions volcaniques, des sources thermales d'une autre nature et dont l'eau aurait une origine interne; rien ne prouve, je l'ai rappelé déjà, que les énormes dégagements de vapeur d'eau, par lesquels se caractérise le volcanisme extérieur, proviennent d'infiltrations superficielles."

Unlike Daubrée, however, he does not regard the circulating waters of superficial origin as responsible for ore deposition. In this matter he follows Elie de Beaumont fairly closely. He attaches much importance to the intimate connection between ore deposits and igneous rocks; and, like Pošepný, regards the metals of the former as arising originally from very deep-seated or barysphéric sources. According to him the initial process

"a dû être essentiellement une réaction de voie ignée, accomplie en profondeur et sous pression, en présence de la vapeur d'eau et des métalloïdes, chlore, soufre, phosphore, bore, arsenic, carbone, etc., dont les reproductions synthétiques ont montré les facultés minéralisatrices."

"En résumé, nous nous trouvons donc en présence d'une opération métallurgique, effectuée sous pression et avec le concours de principes volatiles, où l'eau devait dominer, sur une scorie silicatée, renfermant, à côté de ses éléments à peu près constants, des proportions très variables de certains métaux exceptionnels. Les produits de cette opération métallurgique sont les roches cristallines et les minerais."

"Quand on a conçu la première idée des opérations métallurgiques, qui viennent d'être supposées, on a été bientôt amené à reconnaitre,

dans leurs produits les plus différenciés, deux types extrêmes, qu' Elie de Beaumont définissait : les unes, fondus comme les laves d'épanchement; les autres, cristallisés dans l'eau, ou sublimés, comme les minerais ordinaires des filons concrétionnés. Il en résulte une distinction fondamentale entre le mode de formation de ces deux catégories de substances, auxquelles nous venons d'attribuer une origine commune : une coulée de basalte et un filon de galène."

He regards as of vital importance in the theory of ore genesis the fact of the continuity of granites with pegmatites and the associated vein rocks on one hand, and the continuity of basic rocks with their segregations on the other.

"Aux deux extrémités de la série pétrographique, avec les roches les plus acides comme avec les plus basiques, avec les granites comme avec les péridotites, nous voyons s'isoler des produits métallifères et nous sommes conduits à attribuer le rôle principal, dans leur mode d'isolement, à l'abondance plus ou moins grande des métalloïds volatils ou minéralisateurs (eau comprise): abondance, qui est elle-même function de la profondeur et de la pression. Très faible parfois et, spécialement, sur les fonds de creuset basiques, où les actions de simple ségrégation ignée ont été poussées à l'extrême, cette action peut devenir ailleurs assez grande pour former les types ordinaires de filons hydrothermaux.

Dans le premier cas, la relation du minerai avec la roche est absolument intime; le minerai fait partie intégrante de la roche; il est un des éléments de sa constitution (inclusions); ou, tout au plus, il s'est concentré en certains points du magma (ségrégations), et à son contact (gîtes de départ). Dans l'autre, le métal, entraîné par les minéralisateurs à travers les circuits hydrothermaux, peut s'être écarté plus ou moins loin suivant le degré de solubilité et la precipitation plus ou moins facile de la combinaison saline, dans lesquelles il était entré. Un métal comme l'étain, dont les combinaisons chlorurées se dégagent à haute température et sont précipitées par le seul contact de la vapeur d'eau, ne peut s'éloigner beaucoup du granite, qui lui donne naissance. Tout autre est le sort des métaux, entrés dans des combinaisons sulfurées, qui se maintiennent aisément en dissolution et ne sont reprécipités que par une réaction chimique, souvent à grande distance."

Although he derives the metalliferous matter of veins directly from igneous intrusions, de Launay thinks that the igneous rocks themselves are capable of obtaining supplies from still more deep-seated sources. To explain the characteristics of metallogenic provinces, he invokes supplies of metal from molten reservoirs in the barysphere, whence they

can rise to shallower levels in the igneous rocks as a result of tectonic movements, and thus come within reach of the zone of vein formation. He suggests that in places the silicate crust is comparatively thin, thus bringing the molten reservoirs in the barysphere within easier reach of the surface, a view of things which, however, it is hardly possible to justify on geodynamical grounds.

CHAPTER XI

TWENTIETH CENTURY

VAN HISE, KEMP, LINDGREN, GOODCHILD, MORROW CAMPBELL AND SPURR. CONCLUSION

THE active speculation that took place during the nineteenth century on the processes involved in ore genesis left very little scope for originality to twentieth century workers, whose useful contributions to the subject have been chiefly in the direction of the accumulation of observational data. There has been, however, during these years of the twentieth century, an enormous amount of controversy on the theoretical side of the subject, and although, as is often the case with controversial topics, the value of the contributions to this controversy cannot be assumed to be proportional to their volume, there is, nevertheless, a serious literature on the question so voluminous that one could hardly hope to review it adequately.

Moreover, it soon becomes clear to one on sifting the twentieth century discussions of this problem, that they have in the main followed the old lines, and have tended in some cases merely to re-discover or repeat old views, the former statement and belief in which had been overlooked in the zealous advocacy of other theories. For that reason, if for no other, it is highly desirable that students of ore genesis should interest themselves in the historical aspect of controversy on this subject, if only to see, from the standpoint of generalities, how small is the possibility of making any strikingly new additions to the theories already advanced. Otherwise, they may find themselves in the unenviable posi-

tion attained by some twentieth century workers, of merely proclaiming as new, views that have been advanced in previous generations, and in some instances widely entertained, although since forgotten. Indeed, as already mentioned, a remarkable feature of the twentieth century developments has been the tendency to bring the changes in the theory of ore deposits full circle, more particularly by the advocacy by J. E. Spurr and others of the ore-magma theory in its extreme form, which is practically a return to the views held by Hutton at the end of the eighteenth century, and later by Fournet, Belt and others.

At the outset of the century, in spite of the very excellent work of C. R. Van Hise, opinion had set strongly in the direction of the igneous theory of origin for metalliferous veins, as represented by the views of such authorities as Pošepný, Vogt and de Launay, already referred to. A prominent and influential supporter of this view that vein deposits are directly due to exudations of water from igneous sources was James Furman Kemp, whose strong and consistent advocacy of this view in his University lectures and papers during the first quarter of the twentieth century had doubtless much to do with the popularization of this theory in the United States. In the same sense should be mentioned the work and influence of W. Lindgren.

It is, however, a rather remarkable fact that, side by side with this growing popularity of the igneous exudation theory as advocated by Kemp, Lindgren and others, there grew up, in the United States, a recognition of the importance of the process of secondary enrichment as explaining the richness of many ore deposits, more especially the copper ore deposits of Montana, Arizona, and other parts of the United States. This process of secondary enrichment is due to the action of descending solutions of surface origin in carrying down dissolved mineral matter from shallower to deeper levels, where it is re-deposited to form rich ore. An overwhelming preponderance of the copper output of the

United States during the first quarter of this century was derived from such secondarily enriched deposits formed by the action of water of surface origin.

Small wonder that, in these circumstances, there should be an attempt made by the igneous exudation enthusiasts to account for secondary enrichment by ascending waters of igneous origin. This, however, was in the main an illogical effort on the part of igneous extremists, who grudged the admission of the action of superficial waters in ore deposition. There can indeed be no doubt as to the immense importance of this secondary enrichment action by waters of superficial origin in producing the rich or so-called bonanza deposits which have supplied so much of the world's metal output; and, as already mentioned, it is a very remarkable fact that the growing recognition of the importance of the process of secondary enrichment, more especially by United States workers, during the first quarter of the present century, should have coincided so exactly with the efforts by Kemp, Lindgren and others to establish in the United States, as firmly as they have undoubtedly established it, the igneous exudation theory of ore deposition.

It should be pointed out, however, that secondary enrichment implies the existence of primary deposits at the depth where it takes place, and it is to the direct formation of these comparatively poor primary deposits that the igneous exudation theory is applied; but the fact of secondary enrichment might have been expected to suggest to these theorists that, in view of the earth's long history, superficial waters might have been not without significance in the past, in the large-scale process whereby even the so-called "primary" deposits referred to had been formed. Among United States workers, however, there has been but scanty recognition of this significance of superficial waters in "primary" ore deposition, as formerly maintained by such authorities as Daubrée, Sterry Hunt, S. F. Emmons and Van Hise.

C. R. VAN HISE

In his paper on *Some principles controlling the deposition of ores* (Trans. Amer. Inst. Min. Eng., 1901, vol. 30, p. 27), and again in his *Treatise on Metamorphism* (U.S. Geol. Surv. Monograph, XLVII, 1904), C. R. Van Hise, like Sterry Hunt, advocated a broad geological theory of the genesis of metalliferous veins. He admitted

> "that igneous rocks are the direct igneous source of some ores, that they are the ultimate source of all ores, and that the heat of igneous rocks is of fundamental importance in the segregation of the ores."

While admitting this, however, he pointed out that ore deposits were in this respect like sedimentary rocks, which in most cases had been worked up more than once during the course of geological history, and that, in any particular case, each of which must be considered in the light of the special conditions of its occurrence, the material of the deposit may have gone through a complex series of movements in the earth's crust since it left its primary igneous source.

For this reason he classified ore deposits, like rocks, as igneous, sedimentary and metamorphic, and held that

> "the source of the metal for an individual district is not to be ascribed a priori to igneous, sedimentary or metamorphic rocks, but can be determined only after an inductive investigation of the facts. The metal of a district may be derived from the late igneous rocks, from ancient igneous rocks, from sediments, from metamorphosed rocks, or from any combination of the above. When the important economic districts of the world are inductively studied, and certain knowledge obtained, I believe that it will be discovered that a great number, if not the majority, of ore deposits, are not the result of a single segregation, but are the accumulated fruits of a great interrupted process of segregation, a part of the metals for the deposits having been worked over many times by the metamorphic processes.
>
> According to this view, a general result of metamorphism and accompanying processes is that many of the secondary rocks are depleted in reference to each metal, and correlative with such depletion, other deposits are formed in which each metal is segregated. . . . The natural view is that the metallic ore deposits of the world are, broadly the accumulated results of the processes of segregation carried on throughout geological time.

While it is held that the metals for very numerous and important ore deposits do not have their immediate source in igneous rocks, it is recognized that ultimately the metals for all ore deposits are probably to be traced back to igneous rocks. Since leaving their original positions, the metals for many ore deposits have been transferred and segregated here and there until they reached the places where they are now found."

Van Hise thus regarded ores of direct igneous origin as but a small and comparatively unimportant class, and his three main conclusions regarding ore deposits generally were as follows :—

1. Those deposited by aqueous solutions constitute the dominant class.

2. The water of the aqueous solutions involved in ore deposition is for the most part of meteoric origin.

3. The metals carried by these aqueous solutions are derived from rocks within the zone of fracture.

On the question of the source of the water involved in ore deposition, Van Hise was very emphatic :—

"I have no doubt whatever that water of meteoric origin forms more than 95 per cent. of such waters, and I think it probable that it constitutes more than 99 per cent., if all ore deposits produced by underground water are taken into account. Mistaken conceptions upon this point frequently have been due to the fact that authors, in considering this matter, often take into account only ores of a part of the metals, such as those of gold or silver, whereas a general statement with reference to the proportion of water of meteoric origin should take into consideration deposits of iron ore and of the other base metals. Even in the case of ore deposits closely associated with igneous rocks, it is believed that the water is dominantly of meteoric origin."

As regards the origin of the waters circulating in the earth's crust, Van Hise held much the same views as Daubrée. He saw no reason for regarding these waters as being in any considerable measure of juvenile origin :—

"The study of underground circulation with reference to artesian waters has shown beyond question that the waters discharged from the great majority of springs is of meteoric origin. Numerous large springs are found both in regions in which there has been no igneous action for a long time, and in regions in which volcanism is now or has been recently prominent. No one would claim that in regions of

the former class the water is of other than meteoric origin. For instance, no one would hold that the waters of the great springs of the Appalachians and the Mississippi valley are derived from any other source. Why then should this be held in reference to the Cordilleran region of the west? . . . . If any one asserts that the metalliferous materials of mineral veins are derived by water circulation from the centrosphere, or are derived from the lithosphere below the zone of rock fracture, I hold this to be a pure unverified assumption, for which there has not yet been adduced one particle of evidence, and opposed to which stand well-known principles of physics concerning the movements of water in minute openings, and the condition of water in the deep-seated zone."

Much of Van Hise's reasoning on the problem of ore-genesis is undoubtedly very sound, based as it is on established facts concerning the operations of geological processes. One of his chief generalizations, namely, that the formation of ore deposits in the condition in which we now find them has involved many stages of concentration, by various processes operating through long periods of geological time, is one that is of fundamental importance, as was recognized by Von Cotta and other older workers. It is in strict harmony with what we know geologically of the history through which the earth's crust has passed, and deserves the careful consideration of all students of ore-genesis who wish to find a stable and satisfactory scientific basis for their theories, although it may lead to some measure of indefiniteness and uncertainty in special cases.

J. F. KEMP

Although Kemp in his later years appears to have been a confirmed magmatic hydrothermalist of the juvenile-water school, some of his papers show that he was not inclined to reject altogether the claims of those who attributed vein-formation to meteoric waters; and yet he seemed very reluctant to give these waters any credit for ore deposition.

In his paper on *The Rôle of Igneous Rocks in the Formation of Veins* (Trans. Amer. Inst. Min. Eng., 1902, vol. 31, p. 169), he emphasized the competence of igneous rocks to supply the materials of veins, and showed clearly that he did not agree with the conclusions reached by Van

Hise. In that paper he contended that mining experience had shown free water to be absent from rocks below moderate depths, and that ground water was not as uniformly distributed as had been supposed. He further claimed that

"the distribution of mining districts could only be satisfactorily explained by the corresponding distribution of igneous rocks, which have been intruded under circumstances favourable to vein formation."

In his Presidential Address to the New York Academy of Sciences in 1905, on *The problem of the metalliferous veins* (reprinted in Econ. Geol. 1906, vol. 1) Kemp seems clearly inclined to favour magmatic water as the chief agent in deposition :—

"We may, however, consider an igneous mass of rock as the source of water even if not of the ores and gangue, and then we have a well established reservoir for this solvent in a highly heated condition and at the necessary depths within the earth. Both from its parent mass and from the overlying rocks traversed by it, it may take up metals and gangue.

In the upward and especially in the closing journey, meteoric water may mingle with the magmatic, and as temperatures and pressures fall, the precipitation of dissolved burdens takes place and our ore-bodies are believed to result. Gradually the source of water and its store of energy become exhausted; circulations die out and the period of vein-formation, comparatively brief, geologically speaking, closes. Secondary enrichment through the agency of meteoric waters alone remains to influence the character of the deposit of ore."

Again, writing on ore deposits at the contact of intrusive rocks and limestones, and their significance as regards the general formation of veins (Internat. Geol. Congress, 1906, reprinted in Econ. Geol., vol. 2, 1907), after pointing out that the action of meteoric waters does not furnish an adequate explanation of the facts with which he has to deal, Kemp says :—

"In contrast with meteoric waters, other waters are believed by many geologists to be given off by the deep-seated intrusive rocks and are generally called magmatic. We are led to the belief in their existence by observing the vast quantities of steam and minor associated vapours which are emitted by volcanoes; by the difficulty of accounting in any other way for the amount and composition of certain hot springs; and by the marked and characteristic association of almost all veins with eruptive rocks.

That intrusive masses, in the form of dikes, sheets, laccoliths and batholiths have been genetically connected with the formation of veins is further brought out by the following general consideration which has hitherto received too little attention. Aside from pegmatites, veins rich enough to be mined and even large veins of the barren gangue minerals are extremely rare phenomena, when we compare the regions containing them with the vast areas of the earth which have been carefully searched for them and which have failed to reveal them. As components of the earth's crust, the useful metals, except iron and aluminium, are extremely rare. Some sharply localized, exceptional and briefly operative cause must have brought them into being. The universal circulation of ground water of meteoric origin fails to meet this test, since if it is effective we ought at least to find veins of quartz and calcite fairly universal in older rocks. In North America, moreover, by far the greater number of veins which have been studied, date from the Mesozoic and Tertiary times. The ore deposits of older date are chiefly of iron and manganese, many of which can be satisfactorily accounted for by the reactions of the surface and vadose region or by magmatic differentiation.

The more one reflects upon these actual relationships, the more one is driven to the eruptive rocks as the one exceptional agent, operating in any event by the contributions of heat and energy, and probably also by emissions of magmatic waters. . . . We are irresistibly led to the conclusion that from the intrusive rock has come either highly heated water gas or highly heated water itself in the closing stages, and that one or both of these have brought to the limestone the silica, iron oxide and alumina for the production of the lime silicates. After the production of the garnet and its associates was well under way, they brought in also the copper and iron sulphides, which are the commonest ores."

At the same time, however, there were other workers in the United States who held that meteoric waters rather than magmatic waters were chiefly responsible for the work of ore deposition. This is well shown by a contribution to the discussion on Kemp's paper by T. A. Rickard, which appeared in the Mining and Scientific Press (1908, vol. 96, p. 872). In this note, Rickard claims that the ground-water zone is shallow, and that a dry zone of indefinite thickness separates it from the abyssal magmatic water zone. He also points out that ore is generally non-persistent in depth, and that ore deposits are intimately associated in position with that of the meteoric (ground) water. This, he contends,

favours the belief that meteoric waters are the chief agents in the formation of ore deposits.

W. LINDGREN

In his attitude towards the problem of meteoric versus magmatic water, W. Lindgren took up much the same position as Kemp, though perhaps he was less zealous than Kemp as a magmatic water advocate. Thus, in a paper on *The relation of ore-deposition to physical conditions* read at the 1906 meeting of the Internat. Geol. Congress and reprinted in Econ. Geol. (1907, vol. 2), with reference to the ore deposits of the Cordilleran region, he remarks:—

"Evidence of the genesis of these ores by hot waters has accumulated rapidly in the last few years. It is believed by many geologists that these hot waters were of magmatic origin. Possibly the zeal of the advocates of this view may carry them too far, and it is by no means denied that important ore deposits may be formed by sedimentary processes or by cold or hot circulating waters of atmospheric origin. No special attempt will be made in this place to advocate the deposition of ores by magmatic waters, although the probability of such an origin will be indicated as a corollary from the classification here outlined."

Again, in his Presidential Address to the Geological Society at Washington in 1907 on *Present tendencies in the study of ore deposits* (reprinted in Econ. Geol., vol. 2, 1907), Lindgren remarks:—

"And so in the study of genesis, the most remarkable tendency during the last few years has consisted in this rapid and widespread, though not unanimous acceptance of the magmatic theory, according to which all igneous magmas contain water and dissolved metals which upon the ascent of the magma into a zone of lessening pressure are given off, penetrate the surrounding rocks, and ascend to the surface as thermal springs. The results would in consecutive order consist of (1) products of igneous differentiation in the magmas, (2) contact deposits at the point where the volatile substances left the magma, (3) deposits by magmatic waters on their way to the surface at greater or less distance from their point of origin and more or less mixed with surface waters."

It will be noted that the above statement by Lindgren follows closely the views that had been advocated for some time previously in Europe by de Launay and other authori-

ties. With reference to the claim made by E. Suess and others as to the magmatic origin of hot-spring waters, Lindgren remarks:—

> "It is impossible to peruse the literature of any country of the last few years without noticing the remarkable spread of this idea. . . . The prevailing theoretical tendencies of the present day may be summed up as follows:—We unanimously agree in seeking the ultimate source of the metals in the igneous rocks. We say that the rarer metals and other substances in aqueous solution emanate from the magmas during and after their irruption into the higher levels of the lithosphere, and that minerals containing these metals are deposited along the pathway of the waters. We assert that heated atmospheric waters may search the congealed rocks, abstract from them a part of the small residues of the valuable metals and deposit them along their channels. We say further that metamorphism, when acting upon these igneous rocks, is a potent factor in favour of further concentration, aided by the moisture contained in the rocks."

Finally, Lindgren concludes his summing up by referring to the concentrating action, at the surface, of atmospheric waters.

Lindgren maintained much the same point of view in his textbook on *Mineral Deposits,* in which, however, he made liberal allowance for the action of meteoric waters in ore deposition. He devoted a chapter of this book to lead and zinc deposits which occur in sedimentary rocks, and the origin of which is entirely independent of igneous activity. Indeed, when, as in this book, Lindgren found himself up against the actual facts, and had to group his deposits, he seemed to waver somewhat in his loyalty to magmatic waters; and although he gave as much scope to their action as he reasonably could, he seemed less confident and happy about their comparative importance than he did in the generalizations he made in the papers referred to above.

W. H. GOODCHILD

Among the more notable contributions to the study of ore genesis during the present century, two by W. H. Goodchild figure prominently, viz., *The Evolution of Ore Deposits from Igneous Magmas* (Mining Magazine, vols. 18 & 19, 1918); and *The Genesis of Igneous Ore Deposits*

(Trans. Inst. Min. Met., 1919, vol. 28, p. 274). In these papers, Goodchild deals with magmatic segregation and differentiation as processes in ore deposition, approaching the subject as a metallurgist and reviewing the evidence in the light of physico-chemical principles, more especially as applied in metallography.

The significance of this metallurgical viewpoint is one that was emphasized quite early on in the history of the theory of ore deposits by various continental authors. Among others, Elie de Beaumont recognized the significance of the analogy between rock magmas and the artificial slags and mattes of the metallurgist; and more recently, L. de Launay has given prominence to this conception of the formation and action of intrusive magmas as a sort of natural smelting operation taking place on a large scale in the earth's crust.

According to Goodchild, the important physico-chemical principles involved in a consideration of this problem are: (1), the principle of mobile equilibrium (Le Chatelier); (2), the thermal effect law (Van't Hoff); (3), the law of successive reactions (Ostwald); and (4) the distribution law (Henry).

He attributes a wide extension and a deep-seated origin to igneous magmas:—

"Primary rock magma is evolved periodically over large areas beneath the earth's crust by direct oxidation of elemental magnesium, calcium, iron, aluminium, potassium, sodium, silicon, etc., the process being powerfully exothermic and expansive, and in the nature of an annulment to the general cooling process of the earth. This primary or mega-batholithic magma so produced is of intermediate acid composition in the silica or petrographic sense, and contains small quantities of water, making the melt at these temperatures a slightly acid mixture or 'acid salt' in the hydrogen or chemist's sense. The bases lime, potash, soda, etc., are considered from first principles to be present in the form of their high-temperature or anhydride forms, in contradistinction to the low-temperature forms commonly present in large proportions in the magmas subsequently produced by partial crystallization differentiation.

Primordial magma commences to crystallize in accordance with the law of increasing basicity (Rosenbusch), and the general process of crystallization differentiation may be summarized as follows:—(1)

preliminary crystallization of pyroxenes and accumulation of the free crystals by gravitative descent in the lower regions of the magmatic bath or reservoir; (2) closed crystallization in the upper regions of the mega-batholiths next ensues, forming a roof of granitic composition.

The initial melt being hydrous, and the separating crystals anhydrous, water and other volatile constituents tend to become concentrated at an intermediate stage of crystallization in the middle zone situated between the heap of free basic crystals on the floor and the granite roof. By concentrating the increasingly watery residues from the overhead crystallization in juxtaposition with the basic crystal heap, certain reactions take place which disrupt the crystals and regenerate them into a new basic magma of basaltic composition. . . . One effect of the primary crystallization on the lines indicated above is to develop internally a tremendous bursting pressure which of itself tends to maintain substances in solution and so partially annuls the cooling factor. . . . The various odds and ends left over from the overhead crystallization become concentrated along with water, and the other volatile constituents into this middle zone more or less in juxtaposition with the regenerated basic crystal heap. In this way the materials are assembled for producing, by varying degrees of mixing and diffusion, a whole suite of subsidiary magmas, which may vary in chemical composition from rhyolitic to basaltic, as well as an automatic mechanism for their partial expulsion from these intercrustal reservoirs. The specialized igneous rocks, important as ore bearers, such as the Cornish granites, the American monzonites, the Scandinavian alkaline rocks, the Bolivian stanniferous andesites, the auriferous andesites of wide distribution, etc., are one and all derived from this intermediate zone, and represent partially enriched magmas."

For Goodchild, therefore, the fundamental magmatic unit or starting point is a mega-batholith, derived from the "metallosphere" by direct oxidation. These mega-batholiths, according to him, may even exceed continental dimensions in areal extent. He has thus no difficulty in explaining the abundance of metals in igneous magmas. All he has to do is to explain their fractionation and concentration, which he does by the process of differentiation and subsidiary magma production on the lines indicated above, a process by which they are injected under their own "bursting" pressure into the tension fractures or other cavities of the superincumbent rocky crust. The most specialized of the magmatic differentiates are the ore deposits, and these are so highly specialized that no two of them are alike.

Goodchild attaches great importance to the action of hydrogen as a mineralizer, acting either alone or in conjunction with electro-negative radicals. According to him, this hydrogen in rock magmas may be generated by the action of ferrous and ferric oxides in the magma.

"The effect of the presence of the two oxides of iron in molten magmas is therefore to convert part of the water returned as such in an ordinary rock analysis into free hydrogen. . . . this reaction, by maintaining free hydrogen in the melt, plays an extremely important part in the chemical mechanics of magmatic stoping and igneous injection generally, together with the collateral metamorphism so commonly produced in the neighbourhood of igneous contacts. . . . The fluidity of an igneous magma is thus of a rather different order from that of such a substance as water or molten anhydrous silicates, since it arises primarily from the extreme differences in the molecular sizes and physico-chemical properties of the constituents. It can perhaps best be conceived as the motion of large and heavy spheres, the silicates and their congeners, moving with respect to one another on small, light, pneumatic ball-bearings, the small gaseous molecules, thus producing a melt of much diminished internal friction. Aquo-igneous fusion is clearly a poor and incomplete description of the phenomenon, since the unique gaseous energy of free hydrogen probably plays as great or perhaps an even greater part than that played by water, while in many magmas other gases such as carbon dioxide, fluorine compounds, etc., appear to have played an important part not only in promoting fluidity, but in stoping the melts into position."

Thus, whereas Dolomieu originally invoked interstitial sulphur to explain fluidity in magmas, while Scrope thought that water was sufficient to account for it, and Daubrée attributed it largely to the action of fluorine and other such mineralizers, Goodchild regards it as in large part due to hydrogenation arising from interaction between the water and metallic oxides present in the magma.

According to Goodchild, just as the metals and metallic oxides react with water at high temperature in magmas, so sulphur and the metallic sulphides react with it, producing sulphuretted hydrogen. This gas tends to become concentrated in the fluid monosulphides, which segregate in a cooling rock magma in much the same way as matte segregates in a smelting operation. Such a "gassed matte" in

a rock magma tends to segregate at the cool margins of the intrusion, and

"has extraordinary penetrating power along rock pores, lines of weakness, or foliation planes in the solid rocks near the margin owing to its compressed state and the cutting power of the hydrogen, which becomes oxidized in the process to form water, giving rise among other things to the appearance of hydrothermal (aqueous) alteration on freezing. The genesis of contact deposits at the margins of batholiths and other igneous injections of the type formed before consolidation of the main bulk of the magma is thus easily explained."

The extensive deposits of titaniferous iron ore associated with anorthosites in North America, Goodchild attributes to the hydrolysis of titaniferous pyroxene, at temperatures close to the freezing range, resulting in the formation of two practically non-consolute magmas.

"on squirting the two layers of magma upwards, there will be a tendency for the lesser one to appear in dike-like form traversing the other, and this is precisely the way in which the ilmenite deposits are related to the anorthosites."

He thinks it probable that occurrences of bands of chromite and picotite in peridotites may also be explained in this way, by the hydrolysis of chromiferous pyroxenes. Indeed, with this wonderful sub-magmatic ore-squirt of his, Goodchild can provide any ore-deposit that may be required. Is a "banket" formation required? Very well! If it be a Rand banket, he provides for us an auriferous pyritic sub-magma, and squirts it into a conglomerate. It must surely be a tele-magmatic squirt in this, as indeed in many other cases, where presumably the parent magma must be far removed from the recipient country rock that has to be "squirted." If it be a banket of the Gold Coast type, then he provides a ferric oxide sub-magma. There is no limit to the versatility of this remarkable mechanism.

"Many of the banded ironstones are probably formed in this way, and if squirted into stratified rocks, such magmatic deposits are apt to present the appearance of having been formed by sedimentary agencies."

Goodchild summarizes his view briefly as follows:—

"To put the whole matter into a single sentence, the formation

of ore deposits from rock magmas is to be primarily ascribed to the hydrogenation of silicate melts, while the great co-ordinating law which so greatly facilitates the interpretation of the phenomena is that known as the Le Chatelier Law, viz., when a factor determining the equilibrium of a system is altered, the system tends to change in such a way as to oppose and partially annul the alteration in the factor."

It will be seen from the above extracts that there is much in common between the views of Goodchild and Spurr (see below) with reference to the injection of ore magmas. Whereas, however, Spurr postulates comparatively dry magmas, Goodchild gives much scope to hydrothermal action. Thus, according to Goodchild:—

"Although the contact-metamorphic type of ore deposit formed by two-phase matte concentration is now found to be of much more common occurrence than was formerly supposed, and the deposits, too, are apt to be of large dimensions and corresponding economic importance, probably by far the greater number of ore deposits are derived from the magmas by the agency of aqueous magmatic solutions liberated at quite a late stage in the freezing process. These magmatic waters are undoubtedly instrumental in the formation of ore deposits of practically endless variety of form and composition. . . openings may be filled by squirting liquid matte into them, or by direct crystallization from a metalliferous solution, or by amorphous precipitation from the solutions with subsequent crystallization accompanied by serious differentiation of the precipitate in detail by solid or semi-solid diffusion, and there are at present no generally applicable criteria for distinguishing with certainty between the alternative methods."

Although in some measure a hydrothermalist, Goodchild is not extravagant in his demand for magmatic water. In his paper read at the Institution of Mining and Metallurgy, he spoke of two per cent. of water as being a large amount to postulate for a magma, and he thought that perhaps there might not be so much. In the discussion which took place at the meeting however, J. W. Evans expressed the opinion that, in the average magma, 20 per cent. by weight of water and other volatile constituents would have been much nearer the truth.

J. MORROW CAMPBELL

Another paper of much interest, especially on account of the discussion it raised, was one by J. Morrow Campbell

on *The origin of primary ore deposits* (Trans. Inst. Min. Met., 1921, vol. 30, p. 3). He, too, starts off on the basis of magmatic differentiation. He assumes that the primary magma from which the rocks of the earth's crust were derived was of intermediate composition, corresponding to a diorite or andesite with 60 per cent. of silica and free from water. It must therefore have been of uniform composition, as silicates are mutually soluble at high temperatures in the absence of water. Differentiation of this primary and homogeneous magma was due to absorption of water, which resulted in the separation of the acidic and basic ingredients :—

" The first step towards differentiation was the absorption of water from the atmosphere, which combined as hydroxyl, liberating ferrous oxide principally. This oxidized largely to magnetic oxide. As more was absorbed, the longer-chain silicate molecules lost silica, which, with hydroxyl, gave rise to silicic acid. This in its turn formed a solution with potash alumino-silicate which appears to be immiscible with or very sparingly soluble in a rock melt containing ferromagnesian silicates and basic felspars."

In this way, by splitting the primæval magma, he obtains a result which corresponds very closely to the facts as regards the igneous rock structure of the earth's crust, viz., a solid roof of granite above with a basaltic layer below. He thinks this took place practically uniformly in the whole outer crust and yielded a basaltic layer of very uniform composition and several miles in thickness all round the globe.

According to him, the effect of water absorption on basaltic magma is to produce diorite and peridotite differentiation products. The ultimate effect of further water absorption on peridotite is to break up the silicates and leave a residue of such minerals as chromite, ilmenite, magnetite, etc.

" Concerning the solvent power of the mother-liquor from granite there can be no possible doubt. The view now advanced is that this power is due to silicic acid. It provides the only rational explanation of the segregation of the granitic constituents from the primary magma, and, with them, the ore-minerals.

The amount of the ore-minerals dissolved out of primary magma must depend upon the amount present in it, the amount of free silicic

acid, and the degree of solubility of the ore-minerals in silicic acid. The metals gold and bismuth appear to be freely soluble, also cassiterite and tungstic oxide. Of the sulphides, those of molybdenum and bismuth seem the most soluble, followed by pyrites, chalcopyrite, arsenopyrite, stibnite, galena, and sphalerite. Platinum is insoluble, chromium oxide nearly so, and titanium oxide sparingly soluble. Fluorides and boric oxide are freely soluble, as well as rare earths, niobates and tantalates."

Morrow Campbell thus attributes a vitally important rôle to silicic acid as the agent by which granitic and metalliferous materials are leached out of igneous magmas:—

"Silicic acid is believed to be the solvent of the granitic as well as the ore-minerals. The latter must be the more soluble since they crystallize out at a later stage and at a lower temperature than the bulk of the silicates."

He rejects the pneumatolytic theory, and claims that there is no evidence of the presence of water in a gaseous state in granitic magmas. He considers that:—

"The evidence in nature is overwhelmingly in favour of tin and tungsten having been transported in a silicic acid medium and deposited therefrom probably by loss of heat, the same solution depositing quartz either simultaneously or at a later stage."

He points out that gold and the primary ores of copper, zinc, lead and antimony are invariably associated with quartz, and considers it likely that gold and the primary sulphides of the base metals have been carried from magmas into veins in silicic-acid solution. He is doubtful as to the mode of combination by which this transportation from magmas to veins was effected, but he favours the view that the metals were transported as definite silica compounds rather than simply as solutions. He infers that the ore solutions which deposited certain richly metalliferous pegmatites must have been highly concentrated and that deposition took place quickly, the process being completed in a short space of time—days rather than years. The ore solutions became more dilute as they passed upward, the low-temperature ores being deposited from much less concentrated solutions than those which deposited the high temperature ores.

To account for certain vein-fissures he examined in Burma, Morrow Campbell adopts Goodchild's view that they are due to bursting pressure in the magma consequent upon increase in the solid specific-volume of minerals crystallizing out during the cooling of the magma.

"As it (the bursting pressure) is operative throughout the whole freezing period of the granite, it is evident that its visible effects will appear in waves, so to speak. The upper portion solidifies first, and the bursting energy is stored within the magma, and appears to develop practically at its surface. When the resistance of overlying rock is overcome, fissures are formed, occupied by mother liquor and quickly filled with solids which seal them completely. Solidification of the magma proceeds downward, and more energy is developed, which, finding no outlet, produces new fissures which may or may not coincide with the old ones, but occur in the same area. The fact that within the same circumscribed limits we have (in Burma) as many as three successive series of wolfram-bearing veins cutting one another proves that the first-formed fissures are filled rapidly and completely. Fissures of the second series are often similarly filled."

Morrow Campbell concludes that:—

"Magmatic mother liquor carries silica in solution at all temperatures and pressures and deposits some of it at every level from the granite to the surface. It also carries various metals in solution, some of which may be deposited at any level from the granite to the surface.

Silica and water are the only invariable products secreted by the mother liquor throughout the whole of its upward passage, and no other substance is invariably present in it.

Fluorides, borates, sulphides, etc., cannot be regarded as other than accidental constituents, and one or more of them may be deposited in the granite, along with tin or along with copper minerals, gold or galena, and may even be carried to the surface in solution, but none is invariably present at any particular level. Surely this demonstrates that these so-called mineralizers do not perform any essential function in transportation. . . . Common sense demands that we should credit the substances essential in magmatic liquid, and invariably present in it, with the principal rôle in ore transportation, instead of attributing this function to various accidental constituents that are not always present in it."

In the discussion which took place on Morrow Campbell's paper, many and diverse were the opinions expressed by the various authorities who took part in the discussion.

C. G. Cullis thought it was difficult to understand how meteoric waters could penetrate the crust to the depth at which magmas arise, and asked, what had become of the chlorine carried by these waters? To this the author replied by saying that

> "The absence of chlorine-bearing minerals from igneous rocks may be due to chlorine combining to form volatile compounds to a much greater extent than fluorine. If this is so, chlorine would be almost entirely given off, whereas fluorine would be largely retained in comparatively non-volatile compounds such as fluorite."

J. W. Evans expressed the opinion that in the formation of ore deposits, water was the great "minister of transport," and that other things, including silicic acid, were "passengers." He regarded water above its critical point and under great pressure, in which condition its volume was not very different from that obtaining in the liquid state, as one of the most active reagents in existence. In this condition, and at sufficiently high temperatures, water was capable of dissolving in large amounts most of the materials of the earth's crust. In reply, the author denied that water above its critical temperature was capable of this solvent effect. He argued that if ore minerals were thus transported, their deposition would cease abruptly when the temperature of the vein fell below 365°C., whereas the evidence showed distinctly that the sulphides of the base metals had all been deposited in veins below this temperature. He argued further that the critical temperature had little or no effect on the solvent power of water for silicates, that this went on at and below 300°C., and that from the resulting liquid quartz and felspar, both may be deposited.

S. J. Speak thought that, in most lodes, the metals all entered at once, but that segregation took place later as a result of decreasing temperature and the nature of the wall rock. He thought also that the nature of the metals in a lode depended mainly on the nature of the magma from which they had been derived. He preferred to attribute deposition to ordinary saturation effects in solution, coupled

with the known absorptive properties of colloids. To this the author replied that, whereas probably all the ore minerals given off by a magma are present in the solution that first fills the fissures, in which they are variously deposited according to conditions, it is certain that a magma continues to give off solutions of base metals after it has ceased to yield wolfram and tin, as shown by Tavoy veins, in which shattered wolfram was cemented by chalcopyrite, while as a result of a second re-opening, pyrrhotite with blende, galena and stibnite penetrated both wolfram and chalcopyrite. At each re-opening, quartz accompanied the ores.

H. C. H. Carpenter expressed the opinion that the author had not made out a case for silicic acid, but had merely shown that silica was soluble in water under pressure and crystallized from it.

E. W. Skeats said that, in the case of the gold-quartz veins of Bendigo, the quartz veins entered at the surface for at least two miles from the junction of the granodiorite, and that instead of the gold being evenly distributed, as it should be on the author's hypothesis, it was sporadic in distribution, forming payable shoots locally.

J. W. Gregory thought that the variety and sporadic occurrence of primary ore deposits showed that they were due to some exceptional rather than to a ubiquitous agent, and for that reason he doubted if silicic acid had acted as a transporting agent as the author had suggested. To this the author replied that, as a general rule, silicic acid does not encounter ore-minerals, as shown by the fact that barren quartz is commoner than ore-bearing quartz. This he explained by saying that whereas metals were originally disseminated through a large mass of magma, they have since become concentrated in a shallow zone near the surface. The repeated magmatic absorption and concentration of the metalliferous rocks has resulted in a sporadic distribution, so that, even with a ubiquitous solvent, the tendency would be towards a more and more sporadic rather than a disseminated distribution of ore-bodies.

E. Halse remarked that, in many cases, quartz was not as abundant in veins as it should be on the author's silicic acid hypothesis. On the contrary, sulphur had clearly been the carrier in many cases.

J. E. SPURR

The hydrothermal theory, as advocated by Kemp and Lindgren, with emphasis on the probability of a magmatic source for the water involved in the formation of primary vein deposits, held practically undisputed sway in the United States for many years, and became so firmly established that its advocates gradually extended its application to many of the vein deposits which Lindgren and others had regarded as entirely independent of igneous activity.

In recent years, however, chiefly through the writings of J. E. Spurr, a development towards a more extreme form of the igneous theory of vein deposits seems to have gained ground. This development, as already pointed out, is a return to the views of the dry-magma school of theorists represented in former generations by Hutton, Fournet and Belt.

As early as 1898, Spurr had worked on the Yukon gold-quartz veins, and explained their origin as a variation of pegmatites, due to the action of

> " highly heated water as an end product of granitic differentiation, this magmatic water being the ore solution."

Still later, in 1907, we find Spurr advocating the Zonal theory, according to which, it is claimed

> " that in the final pegmatitic residue of a magma, many metals are present, and that with decreasing temperature each is separately precipitated, the deep-seated gold-quartz ores representing one of the deepest zones, close to the magma."

Apart from the gold-quartz veins and ore-bearing pegmatites, which were regarded by him as intrusive, he continued to regard the residual pegmatitic ore solutions as magmatic waters.

By 1923, however, Spurr had decided to dispense with magmatic waters altogether, and declared himself as an ore-

magma extremist. In his own words, this ore-magma theory

> "substituted for magmatic waters, highly concentrated and dense magmatic residues; for long circulation, a definite stage of injection; for fissures already open, the forcing open of fissures in many cases by the ore magma; for slow accretion of precipitation, simple crystallization from an injected ore magma."

We see then, that whereas Vogt and others had invoked ore magmas chiefly in connection with basic segregations, Spurr boldly extends them to acid differentiates such as pegmatites, and explains practically the whole galaxy of metalliferous vein types as dry-magma differentiates. This leaves little or no scope for magmatic water as an agent in ore deposition, and simplifies the problem in many ways for those who can accept it.

In his work entitled *The Ore Magmas* (2 vols., 1923) Spurr deals very fully with the problem of ore-genesis, and grapples courageously with the geodynamical as well as with the petrographical aspect of the problem. Referring to pegmatites in which metallic minerals such as pyrite, chalcopyrite, molybdenite and gold are frequently found he remarks that such pegmatites may show all gradations from a quartz-felspar to a pure quartz rock.

> "It is difficult to choose between the words 'dike' and 'vein' for these types. Therefore, I propose to call these borderland types 'vein-dikes'; they are intrusive, but the intrusive magma differed from that typical of the usual igneous rock . . . . As to temperature, artificial formation of granitic minerals, and other criteria, indicate that granites and pegmatitic granites crystallized between 575 and 800°C; while the pegmatites and related (pegmatitic) quartz veins crystallized from 575° to somewhat lower temperatures."

With reference to the mode of injection of mineral veins, and the nature of ore magmas or solutions, Spurr gives several North American examples, including an occurrence at the Mandy Mine, Schist Lake, where there is a sulphide lens in schist consisting chiefly of massive chalcopyrite and blende with very little quartz, and showing a sequence of (1) coarse quartz and pyrite; (2) fine-grained blende with streaks of cupriferous pyrite, "the two intimately drawn out and interstreaked by flow as if in a stiff paste"; and (3) high-

grade chalcopyrite with streaks and bands of blende " evidently the result of flow."

" Microscopic sections show that the ore has not been crushed or strained.

Therefore, both main periods of ore deposition, first of blende and later of chalcopyrite, were intrusions of plastic sulphides. . . .

Last of all the ore stages in the Mandy Mine is a little blende and much pyrite, with about 50% quartz; this evidently was deposited from a thinner and more aqueous solution. . . .

Therefore, I conclude that vein-forming solutions, whether at great depths, in the intermediate depths, or close to the surface, may in some cases be highly concentrated, and intrusive as veins or ' vein-dikes.' But vein solutions also have the property of intimate penetration and replacement of rocks traversed, indicating in these cases much thinner solutions."

As regards the mechanism of igneous intrusion, Spurr states that he has found no field evidence to support the notion that it takes place by " roof-stoping " and assimilation. He thinks intrusion is effected by passage of magmas along fractures and dislocation in the earth's crust. He attributes an inherent expansive force to magmas, the pressure arising from which enables them " to thrust up and aside miles of rock."

With reference to Cordilleran uplifts, he thinks the Tertiary magma eruptions around the Pacific are of a single general type, and attributes this to the probability that the whole of the Pacific floor is underlain by magma of intermediate composition, which tends to flow outwards under the floor of the continents, maintaining the latter in a state of emergence. One consequence of this is that the ocean floor is thrust against the continental margin, with folding and faulting effects. Another consequence is a uniformity in type of magmas and ore deposits around the Pacific, the Nevada type of Tertiary ore deposits being characteristic of circum-Pacific regions.

As regards the sequence of eruption in ore magmas, he gives various examples, chiefly Mexican, and remarks:—

" These examples illustrate the law of metal sequences . . . A, tin, molybdenum, tungsten, etc.; B, gold; C, copper; D, auriferous

and argentiferous pyrite and arsenopyrite; E, zinc; F, lead; G, silver; . . . Zones A & B are associated with coarsely crystalline igneous rocks; C & D with finer holocrystalline or coarse porphyritic intrusives; E & F typically with fine textured porphyries, or immediately adjacent igneous rocks are lacking.

A reversal of sequence is obtained by deposition during a rising temperature, due to slow upward igneous migration in depth. Thus at Aspen we have the sequence 1, barite; 2, rich silver ores (zone G); 3, lead and zinc (E & F). The Tiro General vein in Mexico similarly shows 1, zinc (E); 2, copper (C). . . . Where the magma reaches the surface, it apparently arrives there with no loss of heat; and the cooling is so sudden that, when ore magmas penetrate these rocks, the ores of the different vein zones are deposited in nearly the same vertical range, one on the other in quick succession; and are so effectively superimposed that they mingle and may be termed telescoped. Fumarolic deposits in fissures in volcanoes actually show the whole range of the metals, from tin to lead."

According to Spurr, the positions of veins, veindikes and dikes are governed by fissures. Where they are associated, the igneous intrusion is, as a general rule, followed quickly by ore deposition, and this he regards as proof that the ore deposition is a phase of igneous activity. From this general rule, and the local conditions prevailing, he infers that the lead-zinc ore deposits of Missouri and the copper deposits of Michigan are of magmatic origin.

He regards the metals as having a localized distribution in the earth's crust at considerable depths, and in this way accounts for the abundance of copper in Arizona, while in other parts of the Pacific region where similar igneous rocks occur, these contain little or no copper. The Arizona magmas are regarded as having arisen in a part of the crust where copper occurs abundantly and has been brought up by the magmas.

It is to this heterogeneity in the earth's crust as regards the distribution of metals that Spurr attributes the linear extension of the Mexican-Nevada silver camps for a distance of over 2,500 miles, cutting across mountain ranges and paying no regard to tectonic features :—

"These straight-line geometrical metal zones, therefore, probably belong to the stable under-earth, below the zone of magmas, which stream landward from the ocean basins."

Spurr allows practically no scope to meteoric waters as agents responsible for the formation of " primary " mineral veins, and regards their action as limited to comparatively superficial effects such as weathering, mechanical concentration, deposition from surface waters and secondary enrichment. His view that there are two great classes of ore deposits, one arising from the concentrating effect of atmospheric agencies at and near the surface, the other arising from magmatic differentiation, is closely similar to the view held by Fournet, as expressed in a quotation already given from one of the papers in which Fournet states his mature thoughts on this subject.

Spurr's contributions to the study of ore deposits have given much stimulus to discussion on this subject. The ore-magma theory has been the subject of much heated controversy in recent years in the United States, where the magmatic-water theory had for so long held fairly easy sway. Like all theories, it has been pushed to somewhat ridiculous extremes. It has become such an obsession with some workers as to make them imagine that ore magmas exist even where there is no evidence of igneous action in any shape or form. In this respect, however, they are merely repeating a fallacy of the magmatic water theorists, who have long imagined that magmatic waters could be assumed to exist anywhere in the earth's crust where they were required to do the work of ore deposition. Indeed, once a theory is admitted, it becomes easy for an irrational worker to push it to ridiculous extremes, a mental weakness for which the cultivation of the scientific habit of mind is the only remedy.

CONCLUSION ON ORE GENESIS

From the foregoing account of the views expressed by writers on the genesis of metalliferous veins during the present century, it is clear that there is hardly less variety of opinion among the various authorities at the present day than there has been at any stage of the history of this controversy. On the whole, however, the igneous theory in

one form or another holds the field at the present time, and until recently, there has been a great majority in favour of the belief in juvenile aqueous exudations given off by comparatively wet magmas. Latterly, there has been some defection in the ranks of these believers, owing to desertion in favour of the more extreme or ore-magma phase of the igneous theory as advocated by Spurr.

Among the more recent advocates of a deep-seated or baryspheric shell of metalliferous sulphides in the earth's crust is V. M. Goldschmidt (Zeit. Elektrochem., 1922, vol. 28, p. 411), who postulates an outer silicate crust down to a depth of about 750 miles, below this an intermediate zone of sulphides and oxides down to a depth of about 1,800 miles, and below this a central core of nickeliferous iron.

A somewhat similar view of the structure of the earth is advocated by J. W. Gregory, who, however, puts the metalliferous sulphide zone at a much shallower depth. In his *Economic Geology*, Gregory gives an illustration showing a sulphide zone at a depth of from forty to fifty miles in the earth's crust. From this zone he suggests that, by uplift in the superincumbent lithosphere, peaks can rise to a depth of thirty miles or so; and that, at this depth, it comes within the influence of water vapours, which are thereby enriched with metalliferous matter from the sulphide zone, and which carry upward their load of mineral matter until they reach the zone of condensation at depths of from six to twelve miles.

These fanciful speculations by Goldschmidt and Gregory are not essentially different from those put forward by Descartes, De Launay, Pošepný and other earlier workers. We have no substantial evidence to support this notion of the existence of a baryspheric sulphide zone, and much could be said against its probability. However, even if it were necessary to assume the existence of such a sulphidic metalliferous zone at the depths postulated by Goldschmidt and Gregory, we may safely infer that, at these depths, it would be effectively tucked away in the earth's

interior, well out of reach of the processes involved in lode formation. Gregory's reason for postulating such a zone appears to be that there is insufficient metalliferous matter in igneous rocks to provide the metals required for the formation of lodes. On that account he is unwilling to admit that they can arise as ordinary igneous exudations; while at the same time he seems to assume that exogene sources of supply are inadequate for the purpose.

In dealing with specific instances of ore genesis, however, Gregory makes liberal allowance for the operation of exogenetic processes as proximate agencies in ore deposition. For example, he advocates a placer origin for the Rand banket, whereas for the Mansfield copper ore deposits, he holds that the copper minerals were deposited contemporaneously with the sediments in which they occur.

From a geological point of view, indeed, the insistence on a deep-seated or baryspheric shell of metalliferous sulphides to explain the origin of metalliferous veins is very far-fetched; for we have no satisfactory evidence that, excepting basalts, etc., any of the rocks known to us by observation in the earth's crust can safely be assumed to have been formed, in the condition in which we now know them, at depths exceeding ten miles or so. Basalts traverse the crust for comparatively long distances through fissures in the superincumbent cover of granitic, metamorphic and sedimentary rocks; but as we know very well, apart from the ordinary rock elements, the metalliferous content of these basalts and their near relatives is very meagre indeed. It is in association with rocks of comparatively shallow origin in the crust, namely, the sedimentary and metamorphic rocks and the granites intrusive in them, that we find metalliferous veins.

A consideration of mineral deposits as a whole, and the nature of the concentrations represented by deposits of economic importance, enables one to get the problem of the genesis of metalliferous veins in truer perspective than it can be got by dealing with these veins alone. If we limit

our attention to the commoner oxides of the outer earth's crust, including silica, alumina, iron oxides, lime, magnesia, soda, potash, water, titanium dioxide, phosphorus pentoxide and manganous oxide, which probably account for about 99½ per cent. of the outer solid crust, we find that the economically important products in which these oxides figure are almost entirely exogene concentrations, i.e., due to processes operating superficially on the earth's crust, and downwards into it. (See summary on *The Geochemistry of Mineral Resources* by T. Crook; Journ. and Proc. Inst. Chem., 1933, Part 2, p. 122.) If this be the case for the commoner elements, is it likely to be otherwise as regards the scarcer metallic and other elements making up the remaining 0·5 per cent. of the outer crust?

In fact, if we make an overall estimate of the percentage of mineral concentrations due to exogene processes, we find that they account for not less than 85 or 90 per cent. of the present annual value of the world's mineral output. It is doubtful if more than 1 per cent. or so can be attributed to igneous segregations, while, in the writer's opinion, there is at least some uncertainty as to whether exogene processes have not played a prominent part even in the remaining 10 per cent. or so of the concentrations to be accounted for.

We see, therefore, that as regards the commoner elements of the earth's crust, there is an overwhelming case for the importance of exogene as compared with endogene processes as agents of concentration in the formation of economic mineral deposits. If we adopt the safe policy of arguing from the known to the unknown, we shall be led to doubt the efficacy of igneous processes as agents of concentration in the large measure claimed by modern igneous theorists.

We may, indeed, very well agree with Gregory that there is insufficient lode-forming metalliferous matter in ordinary igneous rocks to meet the requirements of the igneous theory of lode-formation; but we need not on that account resort to the barysphere for the required supplies.

They exist in the superficial cover of exogene debris and its deep-seated metamorphic equivalents, partly in a disseminated and partly in a concentrated form. It is therefore unnecessary to postulate baryspheric sources of supply, and it is equally unnecessary to assume that the supplies have come directly from intrusive granites and other such magmas. It seems far more likely that the intrusive granites have captured any supply they may have of lode-forming metalliferous matter for the most part from the rocks they have invaded. If so, the chief effect of igneous action in connection with lode formation has been to cause resurgence of water and metalliferous matter in the invaded rock, with a re-distribution and fresh deposition of this matter along contacts, in fissures, and as replacements in the superincumbent rocks.

In this connection, it should be remembered that, as yet, there has been no satisfactory solution of the problem of the origin of granites occupying fold-cores, although we know that they are almost certainly of comparatively shallow origin. It is, indeed, far from improbable that they represent, at least in part, fused metamorphic and other rocks which have at one time passed through the exogene phase. If so, any metalliferous matter they resurge and concentrate is merely borrowed from exogene sources.

Even, however, if we assume that these fold-core granites are re-melted portions of the continental sial, we must allow that any volatile sulphidic and other lode-forming material in them would have been given off on their first or earlier consolidation, and it is hardly reasonable to assume that they would be able to yield a second crop on further melting. This is a point of some importance in its bearing on the problem generally, and one that seems to escape the notice of igneous theorists when they are explaining ore genesis.

When the first consolidation of the earth's crust took place, the volatile sulphidic and metalliferous matter must have been unloaded from the primary magmas for the most

part, and precipitated in the first-formed exogene deposits. These precipitates represented the primary stock of lode-forming metalliferous matter, the re-distribution of which was to form an integral part of the subsequent dynamical history of the earth. During geological times this primary stock of lode-forming matter has gone through many vicissitudes. It has passed in varying circuits through sedimentary, deep-seated metamorphic and secondary igneous phases; but in so doing, it has merely been going round the geochemical cycle in a comparatively thin outer shell of the earth's crust.

It was considerations such as these, and a belief in the comparatively shallow origin of intrusive granites in the earth's crust, that led the present writer many years ago to reject the assumption of a baryspheric origin for metalliferous veins, and also the juvenile-water hypothesis. It seems likely that, as the significance of geodynamical factors and earth history comes to be more fully understood by students of ore genesis, they will realize more and more that the origin of metalliferous veins is associated with comparatively superficial crustal changes, in which meteoric waters rather than juvenile waters have played the dominant rôle.

In conclusion, one may perhaps venture to prophesy that, if ever petrology develops into a definite and independent science, enlarging its scope to include all rocks, as inevitably it will if there is to be any substantial progress, there will be a corresponding development towards a theory of metalliferous veins which will recognize the comparative shallowness of the crustal circulation which, during the lapse of geological time, has been involved in the changes that have affected the structure of that portion of the earth's crust known to us by observation. Along with this development, it is reasonable to expect that there will be a growing recognition of the inadequacy of igneous magmas as direct and self-sufficing sources of supply of metalliferous and other material for lode formation.

CHAPTER XII

THE RISE OF PETROLOGY

In this chapter an account will be given of the growth of ideas in connection with the study of rocks up to and including the first half of the nineteenth century, by which time the general groundwork of petrology may be regarded as having been built up.

OLDER WORKERS, UP TO HUTTON AND WERNER

As already mentioned, the Greeks knew very little concerning minerals. Their species were few in number (sixteen or so), and were imperfectly distinguished by superficial characteristics. They took no interest in rocks apart from minerals; indeed, it was customary to treat the study of rocks as a part of mineralogy down to the end of the eighteenth century, and as late as 1822 (see below), Haüy dealt with the subject of rock classification in his *Traité de Minéralogie*.

The Romans and Arabians made no advance on the knowledge of the Greeks as regards rocks; and not until the time of Agricola (George Bauer), in the sixteenth century, did it become recognized that rocks formed a distinctive class of mineral substances. Agricola distinguished them from ordinary minerals by their heterogeneity, and indicated that the different individual minerals of which they were composed could usually be separated by mechanical means. Agricola's views appear to have held sway for a long time, and not until the eighteenth century does there appear to have been any progress in the study of rocks.

In his *Systema Naturæ* (1736), C. Linnæus classified the materials of the mineral kingdom as rocks (*petræ*),

minerals including ores (*mineræ*), and fossils (*fossilia*), and later (1768) he divided the rocks as follows:—(1) humose, (2) calcareous, (3) argillaceous, (4) arenaceous, and (5) aggregated. The last of these divisions (*petræ aggregatæ*) included granite, gneiss, porphyry, amygdaloid, etc., which are characterized by distinctive textures.

In his treatise on Mineralogy, first published in Swedish in 1747, and later in his *Systema Mineralogicum* (1778), J. G. Wallerius classified minerals as (1) earths (*terræ*), (2) stones, including rocks (*lapides*), (3) minerals, including ores and metals (*mineræ*), and (4) fossils (*concreta*). The *lapides* he divided into five groups, one of which consisted of *saxa* (rocks), these being subdivided into (a) *saxa mixta*, including typical igneous and metamorphic rocks in which the constituent minerals were intergrown with one another (interlocking texture), and (b) *saxa aggregata*, such as sandstones and conglomerates in which the constituent particles were mechanically aggregated. The different species of rocks were defined according to their mineral composition. Wallerius thus used texture as a basis for his main division of rocks, and mineral composition as a basis for the distinction of species.

In *An Essay towards a System of Mineralogy* by A. F. Cronstedt, published in Swedish in 1758, translated into German in 1760, and into English in 1770, minerals were classified as (1) earths (including all kinds of stones or fossils not inflammable, saline or metallic), (2) salts, (3) inflammables, and (4) metals. He dealt with rocks in an appendix, dividing them into rocks proper (*saxa* or *petræ*), petrifactions and natural slags. Of the rocks proper, he distinguished two groups, namely, (a) compound rocks (*saxa composita*), consisting of different substances so exactly fitted and joined together that no space or cement can be perceived between them, and (b) conglutinated rocks (*saxa conglutinata*), in which the particles are cemented together. Cronstedt's grouping of minerals was followed by Werner (see below). In his treatment of rocks proper (*saxa*), it will be noted that

Cronstedt followed Wallerius as regards mode of grouping.

Other Swedish workers, notably D. Tilas and T. O. Bergman, attached importance to rocks from the geological rather than the mineralogical point of view, and classified them with reference to their bearing on stratigraphy. In this connection, Bergman's *Physikalische Beschreibung der Erdkugel* (1769) was a work of much importance as giving a geological application to the study of rocks. Werner's adoption of Bergman's views had undoubtedly a strong formative effect in establishing the practice of classifying rocks on a geological basis, which practice became confirmed in the nineteenth century, in spite of the contrary pull of the petrographers, and has persisted to the present time.

Bergman was impressed by the definite sequence in stratigraphical arrangement of the rocks of the earth's crust, and grouped them accordingly, in an age series, as follows:—

(1) Primitive rocks, made up of chemical precipitates.
(2) Flötz rocks, made up of mechanical sediments.
(3) Transported rocks.
(4) Volcanic rocks.

In the main, this classification on a geological-age basis, initiated by Bergman, was adopted by Werner (see below), who also adopted Bergman's views concerning the aqueous origin of primitive rocks, including basalts and other rocks which were later proved to be of either igneous or metamorphic origin.

The rivalry between descriptive petrographers and geologists as regards the basis of rock classification, which was such a marked feature of the later development of rock studies in the nineteenth century, was thus begun before the time of Werner. The mineralogists and descriptive petrographers based their classifications on the intrinsic characters of the rocks, i.e., those ascertainable by the study of specimens; whereas the geologists based theirs on extrinsic characters, i.e., those depending on geological relations such as mode of occurrence and genesis.

An early example of the effort to establish a stable and non-controversial basis of rock classification, and one which represented a sort of compromise, was that of Karl Haidinger, who, in his *Systematische Eintheilung der Gebirgsarten* (1787), divided the rocks he described into two main groups, viz., primary and secondary. His statement of the problem of rock classification is of interest and well worth mentioning :—

" Die Aufgabe ist : Eine genaue und natürliche Klassifikation der Gebirgsarten, woraus unsere Erdrinde besteht, nach ihren Geschlechten, Arten und Abarten, zu werfen, . . . ; wobei zugleich zu bemerken, in welcher Gebirgsart dieses oder jenes Metall am häufigsten und gewöhnlichsten angetroffen werde, und glaubwürdige mineralogische Wahrnehmungen anzugeben sind, durch welche die Richtigkeit der Eintheilung sowohl als der übrigen Sätze bewiesen und bestättiget wird."

A. G. Werner, whose *Kurze Klassifikation und Beschreibung der verschiedenen Gebirgsarten* appeared in 1786, regarded geognosy, including the study of rocks, as a branch of mineralogy, which subject he divided into five branches, namely, (1) oryctognosy or mineralogy proper, which was concerned with the description and classification of minerals, (2) chemical mineralogy, (3) geognosy, (4) geographical mineralogy, and (5) economic mineralogy. Although Werner thus treated geology as a branch of mineralogy, he was a keen enthusiast in geological studies, a fact which enables one to understand the paradox that while he did so much for the study of rocks in the purely petrographical sense, and while he regarded geology as a branch of mineralogy, in classifying rocks he consistently preferred a first broad grouping of them on a geological rather than on a mineralogical basis. It was indeed due chiefly to his work, following the lead given earlier by Bergman and other Swedish workers, that the study of rocks came definitely to be regarded as a branch of geology. The tradition he established in classification of making a first board grouping of rocks on a geological basis has persisted, although a change was made later to a genetic in preference to a stratigraphical basis.

As interpreted by Brochant de Villiers (*Traité élémentaire de Minéralogie suivant les principes du Professeur Werner,* 1800; 2nd edition 1808) Werner separated simple minerals from rocks, and for the purposes of classification considered them apart. Simple minerals he divided into four classes, these being further divided into genera and species after the fashion introduced by Linnæus and followed by other authors, including Wallerius and T. Bergman. These classes were: (1) earths and stones, (2) salts, (3) combustible minerals, and (4) metals. Rocks, which were usually mixtures of simple minerals, he divided into main groups not according to their nature and texture, but, following Bergman, according to geological age, as follows:—

(1) Primitive rocks (Ur-gebirgsarten).
(2) Transitional rocks (Uebergangs-gebirgsarten).
(3) Stratified rocks (Flötz-gebirgsarten).
(4) Alluvial rocks (Aufgeschwemmte-gebirgsarten).
(5) Volcanic rocks (Vulcanische-gebirgsarten).

Although Werner classified rocks thus broadly according to relative geological age, he was not unmindful of the other data available as bases of grouping; but he regarded the classification of rocks as having no useful purpose apart from its geognostic (geological) bearings. He taught that the characters by which rocks could be described and distinguished were those relating to their composition, their texture, occurrence and mode of formation. He distinguished simple rocks such as limestone, consisting of one mineral only, from composite or mixed rocks such as granite, porphyry and other rocks consisting of a mixture of minerals.

In dealing with the constituents of composite rocks, Werner distinguished essential (*wesentliche*) from accessory (*zufällige*) minerals. He emphasized texture as a descriptive feature, but rated it at less value for distinctive purposes than mineral composition, because the same kind of rock could have different textures according to the circumstances in which it had been formed. Thus, while realizing

that texture was an indication of mode of origin, he attached no importance to the genetic factor as a broad basis of classification, which is scarcely surprising when one takes into consideration the serious errors of inference he made with regard to the genesis of igneous and metamorphic rocks. He recognized two sharply distinct modes of rock genesis, viz., igneous and aqueous, but he seriously under-rated the significance of igneous action.

The fundamental errors made by Werner concerning the origin of igneous and metamorphic rocks affected his views on ore deposits, which he considered apart from ordinary rocks. The example he set in this matter, which was followed by Von Leonhard and other petrographers and geologists, seems to have been responsible for the divorce that has existed between petrography and the study of ore deposits almost from the commencement of the history of these studies. Although, however, Werner treated rocks and vein deposits as things apart, he gave some consideration to the distribution of metals in connection with the descriptive study of rocks. In his account of granites, for instance, he recognized as characteristic associated metalliferous deposits the ores of iron and tin; other metalliferous minerals recognized by him as sometimes associated with granites were galena, blende, silver minerals, bismuth, copper and molybdenum.

Mention has been made already of the controversy between the Plutonists, who believed in the igneous origin of such rocks as granite and basalt, and the Neptunists, who accepted Werner's view that these rocks were of low-temperature aqueous origin.

While this controversy was running its course during the early years of the nineteenth century, there came forward a new type of specialist who, aware of the scope that existed for the display of energy in ways other than those of controversy, and aware also of the need that existed for more facts concerning the nature of rocks, set to work to study more intensively their mineral composition and the

best way to classify them on the basis of their composition and texture. This new type of specialist (the petrographer) took a keen interest in rock specimens as constituting a separate natural-history group, and independently of their connection with either mineralogy or geology. He found his chief pleasure, not in speculating about the origin of rocks, but rather in the study of rocks for their own sakes. He took all the naturalist's keen delight in describing the different orders, families, genera and species of rocks, and in evolving schemes for their classification.

It was fitting enough that, as a reaction to excessive geological controversy, the study of rocks should at that time take such a turn, for it was clearly necessary, as a means to further scientific development, to build up a body of knowledge concerning the nature and mineral composition of rocks, and the methods available for their examination. The danger lurking in this reaction lay in its tendency to promote the study of rocks as an independent and self-sufficient branch of science, and to detach this study from its important bearings on the principles of geology. This danger soon materialized, and its effects have persisted down to the present time.

At first we find Haüy an active agent in this movement, carrying on the tradition of the eighteenth century of regarding the study of rocks as a branch of mineralogy. At the same time we find Brochant de Villiers and others maintaining the newer Wernerian practice of treating rocks as essentially related to geognosy (geology). In his *Traité élémentaire de Minéralogie,* Brochant included a large section entitled *Traité des Roches* (pp. 559-640), in which he remarks, with reference to " minéraux mélangés ou les roches," that " quant aux minéraux mélangés, ils font l'objet de la géognosie, et ce *Traité des Roches* est une partie de cette science." Elsewhere in the book he mentions that it was customary in France at that time to use the term *géologie* in place of the German term *geognosie* or *gebirgskunde.*

Before long, however, and more especially through the activities of Cordier, Brongniart and von Leonhard, we find petrography attempting to shake itself free from the controlling influence of both mineralogy and geology in an effort to attain independence. The effort succeeded so far as mineralogy was concerned, and from those days onward rocks ceased to find a place in treatises on mineralogy.

Hutton, as we have already seen, had emphasized, and even over-emphasized, the importance of sulphide and vein rocks, and Werner had devoted a special treatise to their consideration. By Werner and Hutton, however, relative geological age and geodynamics respectively were regarded as scientifically more fundamental than the classification of specimens, and presumably for this reason they had no use for a non-geological basis of rock classification. Neither of them, moreover, had any difficulty in assigning a mode of origin to vein deposits.

Werner attributed their formation to deposition from the same surface solutions as had deposited the sediments. His mode of rock classification, however, led him to detach his consideration of vein deposits from that of ordinary rocks. Ordinary rocks he classified on a stratigraphical basis, according to age. Vein deposits would not fit in with this scheme, as their relative geological ages could not be assigned on the basis of stratigraphical relationships in the same way as could those of the stratified rocks. Hence they had to be treated separately; but, as nearly as possible, Werner classified them on the basis of age relations, dividing them into "Lager," which had originated contemporaneously with the enclosing rocks, and "Gänge," which had been formed later than the enclosing rock.

This distinction between "Lager" and "Gänge" was not original with Werner; for even in his day the use of these terms had long been associated with German mining tradition; and Werner, who was very conservative in his temperament, by adopting this mode of subdivision, was merely maintaining and confirming an old-established cus-

tom. This, indeed, he did very successfully, for the division of ore deposits into "Lager" and "Gänge" has come down to the present day, and is essentially the same distinction as that now made by many authors between "syngenetic" and "epigenetic" deposits. It was presumably this distinction, and the inconvenience arising from it, that caused Werner to consider and classify vein deposits as things apart from ordinary rocks. In this practice he was followed by his countryman K. C. von Leonhard, who, perhaps more than any other of the early workers, influenced the development of petrography in the early part of the nineteenth century.

Von Leonhard's treatment of metalliferous deposits in his "Charakteristik der Felsarten," and his omission to classify them as ordinary rocks, had probably much influence in engendering the notion, which grew and strengthened among petrographers in the nineteenth century, that vein deposits were not rocks, and that the purpose of petrography was to deal with those silicate masses and their derivatives which occur either as strata or as congealed masses of intrusive matter injected in a molten condition.

The inconvenience that prevented Werner from classifying vein deposits as ordinary rocks did not exist for Hutton, who regarded them as igneous rocks. We may, therefore, quite safely infer that, if Hutton had been sufficiently interested in the details of rock classification to elaborate a scheme for petrographical purposes, he would undoubtedly have made his scheme complete by including vein deposits, and he would have done this on purely geodynamical grounds.

It is therefore very interesting to consider in this connection the methods of rock classification adopted and followed during the first half of the nineteenth century by French petrographers, who strove hard, but in vain, to establish the simple but fundamentally very important principle that rock classifications should be complete, and that they should cover rocks as a whole, including vein deposits.

Their points of view in this matter, which were mineralogical and petrographical, were quite different from the geodynamical point of view of Hutton, but equally fundamental. Chief among these French workers were Haüy, Brongniart, and Cordier. Haüy, who initiated the movement for completeness, did so on purely mineralogical grounds; whereas the interests of Cordier and Brongniart, so far as classification was concerned, were those of petrographers.

It is indeed quite the most interesting and piquant fact in the history of rock classification that, at the outset of things, Hutton on geodynamical grounds, Haüy on mineralogical, and Brongniart and Cordier on petrographical grounds, should all have exhibited such a definite preference for regarding the term rock in a large sense, and for giving a sufficiently wide scope to petrology to include vein deposits.

Another notable feature about the work of the early petrographers, especially those of the French school, was the strong effort they made to eliminate the controversial geological element from the basis of classification. In doing this, however, they were merely perpetuating a practice that existed in the pre-Wernerian days, of regarding rocks independently of any connection with theories as to rock genesis and basing classification on intrinsic characters. In this respect, while the petrographers of the early nineteenth century did much to advance the study of rocks on the lines of accurate and detailed description adopted by Werner, they merely fell back on bases of rock-classification that had been employed by mineralogists during the eighteenth century.

R. J. HAÜY

In his earliest scheme of classification, Haüy appears to have taken up much the same point of view as that of Haidinger, in explanation of which it should be mentioned that Haüy started in his work on rock classification under the influence and guidance of Dolomieu, and at first gave much scope to geological conceptions in his views on classi-

fication. Later on, however, he attached less importance to geological data, and more to mineralogical and textural data as a basis of rock classification.

It was as Professor of Mineralogy at the Paris Museum of Natural History, and as the officer responsible for the arrangement of the mineral collection there, that Haüy was compelled to look into the bases of rock classification, for the purpose of finding a rational and scientific arrangement of rock types. In a characteristic way he gradually sought out the fundamental aspects of the problem, and on these he built. If, ultimately, he did so with a mineralogical rather than with a geological bias, one cannot be surprised, seeing that his was a mineral collection and he was a mineralogist.

Moreover, the strife prevailing among geologists at that time was eloquent more of the strength of personalities of geologists than of the strength of geological principles. In these circumstances, the only reasonable course available to anyone in Haüy's position was to seek a basis of classification free from entanglement with geological controversy, which basis he ultimately found in mineral composition and texture.

In appendices to the first edition of his famous *Traité de Minéralogie* (1801), Haüy had, following his predecessor Dolomieu, regarded rocks as mineral aggregates, and, as already mentioned, guided by Dolomieu's wide experience as a student of rocks, attempted a classification in which, while having regard to geological data, he sought to secure a non-speculative basis for classification. Dealing with volcanic products in a separate appendix, he classified the remainder of rocks on the basis of texture into:—(1) aggregates which had an interlocking crystalline texture, including the non-volcanic igneous and metamorphic rocks, which were regarded as primary; (2) derived aggregates of comparatively recent and usually sedimentary origin, which owe their hardness to drying, and which are regarded as of secondary or tertiary formation, including clays, shales, marls, shelly marbles, etc.; and (3) discrete cemented aggregates, including conglomerates, breccias and grits.

The interesting point about this first of Haüy's schemes is that, while not departing to any considerable extent from those already in vogue as regards the actual groupings effected, it gave prominence to texture as a basis of grouping; and this, as we shall see, became a dominant feature in the systems of rock classification proposed subsequently by Von Leonhard and others.

As Dolomieu's influence on Haüy's mind receded (Dolomieu died in 1801), Haüy attached less importance to geological considerations, and came more and more to regard intrinsic data as the proper basis of classification. In 1811 he announced (Von Leonhard's Taschenbuch für Mineralogie) his intention of classifying rocks primarily on the basis of mineral composition; and in the second edition of his *Traité* in 1822, he attempted to do this in a very comprehensive way by dividing rocks into seven classes as follows:—

1. *Substances pierreuses et salines* (including phosphorite and fluorite).
2. *Substances combustibles non-métalliques* (including graphite and coals).
3. *Substances métalliques* (including ores of iron, copper, lead, zinc and tin).
4. *Roches d'origine ignée suivant les uns, aqueuse suivant les autres* (including dolerites and basalts).
5. *Roches généralement regardées comme ayant une origine ignée* (including lavas and tuffs).
6. *Matières sublimées* (including sulphur and hæmatite of volcanic origin).
7. *Thermantides* (*substances altérées par les feux non-volcaniques*).

This classification of rocks by Haüy is of special interest to the student of ore genesis, not only as providing a class for sublimation products, but also as making an important class to accommodate metalliferous-vein rocks, for which conventional petrographers have never made provision. He also included in his class of "substances pierreuses et

salines " not only the usual silicate types of rock, but non-silicate types such as rock-salt, fluorite and phosphorite.

Following the Linnean system that was already in vogue among many authors, Haüy sub-divided his classes into orders, genera and species, the division into orders being based on texture. His third class of " substances métalliques " included separate genera for the different metals, the species being mineralogical.

Haüy's scheme influenced greatly the minds of his contemporaries, especially Cordier, Brongniart and von Leonhard. The adoption of mineral composition and texture as petrographical bases of rock classification was due more than anything else to the influence of his work. It is all the more necessary that credit should be claimed for this on Haüy's behalf, on account of the somewhat ungracious comments that have been made at various times concerning his scheme of rock classification, notably by Lossen in Germany (" *Ueber die Anforderungen der Geologie an die petrographische Systematik.*" Jahrb. k. preuss. geol. Landesanst. u. Bergakad. für 1883, 1884) and Whitman Cross in the United States (Journ. Geol., vol. 10, 1902; and *Quantitative Classification of Igneous Rocks,* 1903).

Judged from the standpoint of the principles of classification, and in relation to the narrowing tendencies of scientific petrography during the nineteenth century and later, as exemplified more particularly by the quantitative system of classification of igneous rocks, Haüy's scheme commands respect as an excellent effort to put the classification of rocks as a whole, and not merely igneous rocks, on a basis consistent with sound science and common sense. It struck the note that dominated rock classification in France up to and beyond the middle of the 19th century, more especially through the influence of Cordier, who, in common with Haüy and Brongniart, made his purely intrinsic and non-geological basis of classification broad enough to embrace all rocks, including metalliferous-vein types.

In the foregoing account of the work of Haüy, his mature scheme of rock classification has been included, as his statement in Von Leonhard's Taschenbuch in 1811 so clearly indicated his decision to make mineral composition the prime basis of classification, and he had evidently adopted the scheme set forth in the 1822 edition of the *Traité* long before that work was published. It seems clear, indeed, that he has distinct priority over Brongniart in the matter of emphasizing mineral composition as the primary basis of rock classification, although chronologically the second edition of Haüy's *Traité* was preceded by the publication of a paper in 1813 by A. Brongniart, in which this author made no reference to Haüy's work, except as regards the latter's activity in introducing rock names.

A. BRONGNIART (1813)

The paper by Brongniart referred to above was entitled *Essai d'une classification minéralogique des roches mélangées* (Journ. des Mines, 1813, vol. 34; No. 199, p. 5). It dealt with composite rocks, the rocks proper of Dolomieu, who defined the term rock in a less comprehensive way than Werner had done, excluding Werner's simple rocks, which consisted of one mineral only. While limiting his paper to composite rocks, however, regarding, as he did, the study of simple or homogeneous rocks as falling properly within the province of the mineralogist, he adopted the broad definition of the term " rock " prevailing at that time among mineralogists, who had up to that time been the custodians of rock study.

" J'appelerai ROCHE toutes les grandes masses pierreuses, salines, combustibles ou métalliques qui entrent dans la structure de la terre " (*Op. cit.*, p. 6).

This definition is especially noteworthy on account of its comprehensiveness. It appears to have been carefully worded so as to include all Werner's classes of minerals, and shows clearly Brongniart's intention that his definition of the term rock should be complete enough to include all kinds of mineral masses, and not merely the igneous, sedimentary

and metamorphic rocks of modern petrology. Brongniart recognized the distinction that it was customary to make at that time between homogeneous rocks (e.g. saccharoidal limestone, gypsum, rock-salt, and coal) and heterogeneous rocks (e.g. granite, porphyry and conglomerate). He made a sharp distinction between two different points of view regarding rocks, one of which he referred to as constituting their *histoire minéralogique,* the other their *histoire géognostique*. These two different viewpoints, distinguished so sharply by Brongniart, were precisely those indicated by the present-day usage of the terms petrography and petrology, the former having to do with the composition and texture of rocks, the latter with their geological relations, as Brongniart's statement of this distinction clearly shows:—

"Dans l'un on ne doit considérer que la nature des roches, leurs qualités extérieures, leurs propriétés physiques et chimiques; c'est ce qui constitue leur histoire minéralogique.

Dans l'autre on a pour objet d'étudier le rôle qu'elles jouent dans la structure de la terre, de connaître leurs rapports entre elles et avec les autres minéraux, etc., c'est le sujet de leur histoire géognostique" (*Op. cit.,* p. 7).

As in his definition of the term rock, so again in connection with the basis of classification, he emphasizes the fact that rocks have to be considered from two points of view, viz.:—

"1. Relativement à leur composition, c'est-à-dire, à la nature, à la quantité, et à la disposition des parties qui les composent.

2. Relativement à leur gisement, c'est-à-dire, à la place qu'elles tiennent dans la structure du globe, et à leurs rapports entre elles" (*Op. cit.,* p. 10).

He complained, presumably with reference to the Wernerian school, that German mineralogists, who had adopted a classification on the geognostic basis (i.e. occurrence and geological relations), were opposed to a classification based on mineral composition and texture. He claimed that, as a result of this, the terminology of rock descriptions was incomplete and altogether inadequate for the purpose of a descriptive study of rocks, which required treatment as a

thing apart from its geognostic bearings, in order that the different important types of rocks should be distinguished with sufficient clearness.

"La détermination précise, la description, enfin l'histoire minéralogique complète des roches mélangées, me semble donc devoir être faite séparément, et précéder leur histoire géognostique. Cette détermination établie aussi sévèrement que le sujet le comporte, rendra la description des diverses couches de la terre plus précise et plus claire" (*Op. cit.*, p. 8).

After a consideration of the importance of mineral composition and texture as bases of classification, he was led to conclude that,

"dans la classification des roches mélangées, le caractère tiré de la nature ou du corps dominant, doit être mis en première ligne, et celui que donne la structure doit être placé en seconde ligne, soit pour être employé à former les divisions moins essentielles que celles de l'espèce et du genre, soit pour remplacer le premier lorsqu'il manque" (*Op. cit.*, p. 20).

Although, however, Brongniart thus emphasized mineral composition as a basis of classification, as Haüy had done previously, it was only in the genera and species that he adopted it, his classes being based on texture, as follows:—

Class 1. Roches cristallisées isomères. Parties liées par agrégation cristalline, sans base ou partie dominante essentielle, ni ciment homogène sensible (including granite, syenite and diabase).

Class 2. Les roches cristallisées anisomères. Formées en tout ou en partie par voie de cristallisation confuse; une partie dominante servant de base, de pâte on de ciment aux autres, et contemporaine ou antérieure aux parties qu'elle renferme (including gneiss, schist, calciphyre, amphibolite, porphyry and volcanic rocks).

Class 3. Les roches agrégées. Formées par voie d'agrégation mécanique; un ciment ou pâte postérieure au parties qu'elle renferme (including grit, conglomerate and breccia).

This grouping, especially as regards the textural basis of class distinctions, keeps closely to the lines adopted by Haüy in the first edition of the *Traité*, but divides, still on a textural basis, Haüy's class of igneous and metamorphic

rocks, and includes in its appropriate class the group of the volcanic rocks. At a later date (see below) Brongniart dealt with rocks as a whole, including simple as well as composite rocks, while at the same time adopting Haüy's very sensible and logical practice of including metalliferous vein rocks in the scheme of rock classification.

P. L. A. CORDIER

Contemporaneously with Haüy and Brongniart in France, P. L. A. Cordier gave close attention to the study of rocks, and more especially volcanic rocks. Cordier probably knew as much about rocks in general, both as regards their composition and geological relations, as any other worker of his time; while, on the subject of volcanic rocks, he did work of pioneer value in applying physical and chemical methods to the study of their composition.

Although it is for these original investigations on the composition of volcanic rocks that Cordier is chiefly remembered as a petrographer, his influence among his contemporaries, not only in France, but in other parts of Europe, was felt more in connection with his views on the classification of rocks and the scope of rock studies. These views, which he expounded in his lectures as Professor of Geology in Paris, were, according to Brongniart, originally published, as presented in his course of lectures in 1822, in *Bibliothèque italienne,* vol. 28, p. 376. They were also published in a paper by E. T. Kleinschrod (*Jahrb. d. Min. Geogn.,* etc., 1831, vol. 2, 17 and 559), to whom Cordier lent his manuscript; and another account, presumably based on Kleinschrod's paper, appeared in *Bull. Soc. Imp. Nat. Moscou* (1832, vol. 4, 270).

A fuller account of Cordier's views, as presented in his course of lectures on geology at the Museum of Natural History in Paris, was given by C. d'Orbigny in his *Classification et Principaux Caractères Minéralogiques des Roches* (1848), which was based on notes taken at Cordier's lectures, and appeared in the *Dictionnaire universel d'Histoire*

naturelle, and at a much later date, in d'Orbigny's *Description des Roches* (1868).

As represented by d'Orbigny in the latter's notes, Cordier defined rocks as the constructional materials of the earth's crust:—

"On désigne sous le nom de Roches toutes associations de parties minérales, soit de même espèce, soit d'espèces différents qui se trouvent dans l'écorce solide du globe en masses assez considérables pour être régardées comme parties essentielles de cette écorce et être prises en considération dans son étude générale."

Cordier's classification of rocks is fully in accordance with this definition, for he was too logical in his mental attitude towards vein deposits, and other such materials occurring as masses of considerable size in the earth's crust, to ignore the fact that they are rocks; and in his scheme he appears to have sought above all things to secure accommodation for all kinds of material that came within the scope of the term "rock," defined geologically, as he and others had defined it. His scheme of classification shows that he was strongly influenced by Haüy, whom he followed in making mineral composition the primary basis of grouping.

On this basis he recognized thirty-four families of rocks. The family name was determined as a rule by the predominant constituent, texture being regarded by Cordier as subordinate in importance to mineral composition as a basis of grouping, and serving only for the purpose of making subdivisions of families:—

"La division en familles est fondée sur la base des roches, et les soudivisions dépendent de la structure ou du mode de conjunction des parties constituantes."

The families were subdivided, very elaborately in some cases, into orders, genera and species, and were, moreover, arranged in four large classes.

These classes into which Cordier divided rocks, corresponded closely to the classes into which Cronstedt and Werner had divided minerals. The arrangement was as follows:—

CLASS I. ROCHES TERREUSES.

1. *Roches feldspathiques.*
2. ,, *pyroxéniques.*
3. ,, *amphiboliques.*
4. ,, *épidotiques.*
5. ,, *grenatiques.*
6. ,, *hypersténiques.*
7. *Roches diallagiques.*
8. ,, *talqueuses.*
9. ,, *micacées.*
10. ,, *quartzeuses.*
11. ,, *vitreuses.*
12. ,, *argileuses.*

CLASS II. ROCHES SALINES ACIDIFÈRES NON MÉTALLIQUES.

13. *Roches calcaires.*
14. ,, *gypseuses.*
15. ,, *à base de sous-sulfate d'alumine.*
16. ,, ,, ,, ,, *muriate de soude.*
17. ,, ,, ,, ,, *sous-carbonate de soude.*

CLASS III. ROCHES MÉTALLIFÈRES.

18. *Roches à base de carbonate de zinc.*
19. ,, ,, ,, ,, ,, ,, *fer.*
20. ,, ,, ,, *d'hydrate de manganese.*
21. ,, ,, ,, *de silicate de fer.*
22. ,, ,, ,, *d'hydrate de fer.*
23. ,, ,, ,, *de peroxyde de fer.*
24. ,, ,, ,, ,, *fer oxydulé.*

CLASS IV. ROCHES COMBUSTIBLES NON MÉTALLIQUES.

25. *Roches à base de sulfure de fer.*
26. ,, ,, ,, ,, *soufre.*
27. ,, ,, ,, ,, *bitume gris.*
28. ,, *pissasphaltiques.*
29. ,, *graphiteuses.*
30. ,, *anthraciteuses.*
31. ,, *à base de houille.*
32. ,, ,, ,, ,, *lignite.*

APPENDICE.

33. *Roches anomales.*
34. ,, *météoriques.*

We see from this scheme that Cordier made a separate class of *Roches métallifères* for some of the more clearly defined types of metalliferous rocks. The vein deposits pro-

per, however (*Roches de filons proprement dîtes*), constituted one of two orders forming the 33rd family of *Roches anomales,* the other order consisting of *Roches des grottes et cavernes et des fentes superficielles.*

The division of the families, where division was necessary, was on the basis of texture. In the case of the family of felspathic rocks, for example, there were two orders, viz., *phanérogènes* and *adélogènes.* The subdivision of the orders into genera was also according to texture, three genera being recognized where necessary, viz., *agrégées, conglomérées,* and *meubles.* These genera were divided into species.

The vein rocks forming the genus of anomalous aggregates in the family of *Roches anomales* included many species, each of which was characterized by the predominance of some particular mineral, the chief of these being quartz, calcite, barite, apatite, fluorite, pyrite, chalcopyrite, galena, cerussite, blende, cinnabar, wolframite, cassiterite, chalybite, hæmatite, limonite, etc.

Some of the families were small and comparatively insignificant. Thus, the family consisting of rocks with a base of hydrated silicate of iron had only one species, viz., chamosite (glauconite), of which there were three varieties, viz., ordinary, calcitic and quartzose.

K. C. VON LEONHARD

In his *Characteristik der Felsarten* (vol. 1, 1823; vols. 2 & 3, 1824), K. C. Von Leonhard, who was the foremost authority on rocks in Germany at that time, followed the lead given by his French contemporary Brongniart, who had advocated the study of rocks as a separate subject, and had expressed the view that the description and classification of rocks called for a special terminology which should be independent of geognostic considerations. It was on these lines that Von Leonhard developed the subject of his treatise, in which, as Werner and others had done before him, he did much to promote the descriptive study of rocks on a basis of

more accurate knowledge of their nature and texture, and the use of a more exact terminology.

Whereas Werner, however, had adopted a geognostic basis of classification, and had regarded any other basis as unnecessary, Von Leonhard agreed with Brongniart that it was necessary to study the characteristics of rocks quite independently of geognostic considerations, as otherwise it was not possible to secure an adequate and satisfactory basis for their description and classification. The influence of Brongniart's work is indeed very clearly seen in Von Leonhard's treatise, and it should also be mentioned that Von Leonhard had also had the benefit of intercourse with Haüy, with whose arrangement of rocks in the Paris Museum of Natural History he was quite familiar.

Although in agreement with Haüy and Brongniart so far as the principle of classification on the basis of intrinsic characters was concerned, Von Leonhard differed from them in regarding texture as far more important than mineral composition as a basis of broad grouping. Haüy, as we have seen, after having tried the texture basis, came ultimately to regard mineral composition as the more satisfactory basis, and had made a thorough-going application of this principle in the 2nd (1822) edition of his *Traité*. In this respect, Brongniart took up a less decisive attitude, for while he stated that mineral composition was more important than texture as a basis of grouping, he based his main groups on texture. Von Leonhard went much farther than Brongniart in giving sharp prominence to texture as compared with mineral composition in his classification. He also abandoned the application to rocks of Linnean group names such as orders, genera and species, a practice in which later petrographers have largely followed him; for although we find that such group names as "class," "family" and "species" are still employed in petrography, there is no pretence of biological analogy in the use of these group names at the present time.

Von Leonhard's main groups were five in number, though it should be mentioned that, in his *Characteristik der Felsarten,* the third group of apparently homogeneous (*scheinbar gleichartige*) rocks was treated as an appendix to the group of homogeneous rocks rather than as an independent group. At a later date, in his *Lehrbuch der Geognosie und Geologie* (1835) he had not been able to distribute the rocks of this tentative group and therefore treated it more seriously. His grouping was as follows:—

1. UNGLEICHARTIGE (HETEROGENEOUS) GESTEINE.
   - (a) *Kornige* (granular; including granite, gabbro, dolerite, etc.).
   - (b) *Schieferige* (gneiss, etc.).
   - (c) *Porphyre.*
2. GLEICHARTIGE (HOMOGENEOUS) GESTEINE.
   - (a) *Kornige* (including granulite, crystalline limestones and rock-salt).
   - (b) *Schieferige* (schistose; including talc-, hornblende- and chlorite-schists).
   - (c) *Dichte* (compact; including chalk, limestone, etc.).
3. SCHEINBAR GLEICHARTIGE (NICHT ALS GLIEDER ORYKTOGNOSTISCHER GATTUNGEN ZU BETRACHTENDE) GESTEINE.
   - (a) *Dichte.*
   - (b) *Schieferige.*
   - (c) *Porphyre.*
   - (d) *Glasartige.*
   - (e) *Schlackenartige.*
4. TRÜMMER-GESTEINE (Cemented clastic sediments; e.g. conglomerates and breccias).
5. LOSE-GESTEINE (Uncemented clastic sediments; e.g. sand and gravel).

It was perhaps well that the value of texture as the chief basis of classification should be tried out as fully as Von Leonhard tried it in this scheme; but it would be difficult to evolve a more clumsy and unnatural arrangement on any basis having claims to be regarded as scientific. It suffered from the further disadvantage, in comparison with the French schemes of that time, that it was seriously incomplete, being practically restricted to silicate and sedimentary rocks. That was a feature characteristic of German schemes

from Karl Haidinger and Werner onward. It was doubtless due, as already suggested, to the prejudice established by Werner's geognostic classification and the exclusion of vein deposits as things not classifiable as rocks in his geognostic scheme.

Von Leonhard thus not only abandoned the geognostic principle of classification advocated by Werner, but also failed to adopt the important principle of completeness which was embodied in the schemes of Haüy and Cordier, while his texture basis was also far less natural than the mineral-composition basis recognized so fully by the French authors. This restricted outlook by Von Leonhard as to the scope of rock studies was an unfortunate thing for petrography, as it seems to have had much influence in fixing the ideas of later petrographers in this matter. His attitude of mind is the more difficult to understand, because he shows quite clearly in his *Lehrbuch der Geognosie und Geologie* that he appreciated not only the economic importance of the application of geology to the study of vein deposits, but also the great scientific importance of the bearing of the genesis of vein deposits on rock genesis generally.

It would indeed be difficult to find anywhere a clearer and terser statement of this matter than the one made by Von Leonhard in the section of his *Lehrbuch* dealing with metals and their ores, in which he remarks that the study of metalliferous deposits, and more especially metalliferous veins, is important scientifically as well as practically; that the geognostic-geologic relationships of vein deposits render it very desirable that the phenomena connected with their occurrence and association with rocks should be carefully studied, as they have an important bearing on the structure of the earth, and only by an understanding of these relationships can that structure be adequately understood (*Op. cit.*, p. 755).

With such views as these one would have expected Von Leonhard to make provision for metalliferous deposits; and more especially vein deposits, in his classification of rocks,

but this he failed to do. He did, however, both in his *Characteristik der Felsarten* and in his *Lehrbuch*, after the fashion of the Wernerian school as exemplified by Brochant de Villiers in the latter's *Traité*, indicate in his description of rocks the kinds of metalliferous deposits usually associated with them, and it was this Wernerian fashion that became stereotyped and served as a pattern which was followed in later treatises, although with ever-decreasing enthusiasm.

If Von Leonhard had followed up his excellent statement and appreciation of the geodynamical significance of vein deposits by making adequate provision for them as rocks, as was done by the French school, it would have been difficult for later petrographers to escape from the responsibility of making provision for their consideration and classification as rocks, in however imperfect a way. As it was, his treatment of them was tantamount to an inference that they were not rocks, and the way was thus paved for petrographers to evade altogether the study of vein deposits as distinct from dikes. This way they gladly followed, as vein deposits could not be classified conveniently, and were on that account doubtless regarded as so much impedimenta by the majority of later systematists, whose chief desire was to find a positive geognostic basis for the grouping of silicate and sedimentary rocks. As, however, no such basis seemed to be available for the grouping of vein deposits, these were thrown aside as things of no importance for the purposes of petrography.

A. BRONGNIART (1827)

Shortly after the publication of Von Leonhard's *Characteristik der Felsarten* there appeared A. Brongniart's *Classification et Caractères minéralogiques des Roches homogènes et hétérogènes* in 1827. In this excellent little book, Brongniart included much that he had written in his *Essai sur une Classification minéralogique des Roches mélangées* in 1813; but whereas in that essay he had dealt only with composite rocks, he now extended his account to

include homogeneous rocks, and furthermore, following Haüy and Cordier, he enlarged the scope of his classification to include metalliferous-vein and other such rocks, which, as we have seen, Von Leonhard had excluded from his scheme, although he devoted a chapter to the consideration of their occurrence and association with rocks. Appreciating Von Leonhard's exclusion of geognostic data as a basis of classification, and his adoption of texture as the main basis of grouping, Brongniart continued to base his main groups on texture, but gave much less prominence to this feature than Von Leonhard had done. Thus, while he was in accord with Von Leonhard as regards the recognition of texture as the basis of distinction for his chief groups, he sought to secure the advantage of completeness of scope for rock classification as exemplified by the schemes of Haüy and Cordier.

Brongniart's new arrangement was as follows :—

Class 1. ROCHES HOMOGÈNES OU SIMPLES.
- Order 1. *Roches phanérogènes.*
  - Métaux autopsides.
  - Métaux hétéropsides simples.
  - Métaux hétéropsides combinés.
- Order 2. *Roches adélogènes.*

Class 2. ROCHES HÉTÉROGÈNES OU COMPOSÉES.
- Order 1. *Les roches de cristallisation.*
- Order 2. *Les roches d'agrégation.*

The *métaux autopsides* of Class 1 were divided according to the metals, thus: *zinc* (calamine), *cuivre* (cuivre pyriteux), *manganèse* (manganèse terne), *fer* (pyrite, fer oxidulé, fer oligiste compact, fer oligiste sanguin, fer hydroxide compact, fer hydroxide pisolitique, fer hydroxide oolitique, fer hydroxide limoneux, fer carbonaté spathique).

The *métaux hétéropsides combinés* were divided into *muriates* (selmarine rupestre), *fluates* (fluorite compact), *phosphates* (phosphorite compacte), *sulphates* (celestine, barytine, and alunite), and *carbonates* (giobertite, magnésite, dolomite and calcaire).

If anything at that early date could have established petrography as a branch of study independent of the controlling influence of geology, the co-operation of Brongniart in France and Von Leonhard in Germany, who were both prominent as geologists, might have been expected to have that effect. In fact, however, while Von Leonhard's work continued to influence the growth of petrography in Germany, and many of the terms used by him continue in use at the present day in much the same senses as he used them, his classification was never widely adopted, although certain features of it may be recognized in the broad grouping of rocks in Germany at a later date by Zirkel and Rosenbusch.

In France, Brongniart had much less influence as a petrographer, in competition with Cordier, who, although he himself never published his scheme of classification, was the dominant authority in French petrography during most of the first half of the nineteenth century. It was, indeed, well beyond the middle of the century before the influence of Cordier waned in France, owing to the work of F. Fouqué and A. Michel-Lévy, who, in their *Minéralogie micrographique,* gave an account of petrography which, based as it was on the application of the newer methods of microscopy to the study of rocks, inevitably superseded the work of Cordier. In this enthusiasm for the detailed study of rocks, and more especially igneous rocks, by means of the microscope, breadth of outlook was lost. Fouqué and Lévy's work was cramped by the narrow view, so characteristic of later developments, that petrography is essentially the descriptive study of silicate rocks, a view of things which, since the middle of the nineteenth century, has severely restricted the scientific value of petrography as a branch of study.

C. F. NAUMANN

Twenty years after the publication of Von Leonhard's *Lehrbuch,* there appeared in Germany in 1847 another *Lehrbuch der Geognosie,* this time by C. F. Naumann. In

France and England, the use of the name geology for the science had become firmly established, having been introduced by De Luc and used by De Saussure and many other French authors, and given a strong footing in England by the publication of Lyell's *Principles of Geology*. In spite of this, however, Naumann kept alive the German tradition as represented by Werner and Von Leonhard, and preferred the name *Geognosie*.

Naumann lacked Von Leonhard's thorough knowledge of the composition and texture of rocks, his interest in rocks being of a general rather than a special kind, and concerned not so much the study of rocks for its own sake as for its relation to the science as a whole. His interest in rocks was thus that of a geologist rather than that of a petrographer, whereas Von Leonhard was first and foremost a petrographer.

Naumann applied the name *Petrographie* or *Gesteinslehre* to the study of rocks, regarding it as a definite large branch of geognosy. He regarded rocks from many different points of view and divided the subject into branches, dealing separately with mineral composition, texture, mode of occurrence, systematic description, genesis and alterations. The branch dealing with rock genesis he termed *Petrogenie*, and he distinguished rocks according to their mode of origin as minerogenous, zoogenous and phytogenous.

With all this and more in the way of an elaborate framework for petrography, Naumann's effort at the classification of rocks in general was rather feeble. Discarding the elaborate textural basis of Von Leonhard, he divided rocks into (1) *Crystalline* (2) *Clastic*, and (3) Other kinds (including *amorphous, zoogenous* and *phytogenous*). In the second edition of his *Lehrbuch*, he made a modification of this arrangement, dividing rocks into two main classes, viz., 1. Protogenous (original), 2. Deuterogenous (derived), a mode of division not essentially different from that adopted by Haidinger in 1787. His class of protogenous rocks was

divided into six orders according to mineral composition, viz., ice, haloids, quartz, silicates, ores and coal. These orders were in turn subdivided into families, also according to mineral composition, texture being used only for the purpose of further minor subdivision.

In his treatment of rocks, therefore, Naumann differed fundamentally from Von Leonhard; but, in the changes he made, Naumann went backward rather than forward, and showed no originality of conception. In many respects he reminds one of Werner, whose natural successor he was as a powerful conservative influence in German geology. He clearly had no sympathy with the view advocated by Brongniart and Von Leonhard, that rocks required to be studied independently of their connection with geology. On the contrary he re-affirmed Werner's view that the scientific study of rocks was necessarily tributary to the science of geognosy; and that, although they could be considered from various points of view as regards their nature and genesis, etc., their classification for scientific purposes should be based primarily on broad geological considerations.

Naumann's *Lehrbuch* had a strong formative influence on geological and petrological thought in Germany, where developments arising out of the detailed study of rocks under the microscope left practically unshaken the Wernerian view, fostered by him, that the scientific study of rocks is vitally bound up with geology. These developments, moreover, did not seriously disturb Naumann's broad grouping of rocks as protogenous and deuterogenous so far as Germany was concerned, for it was essentially the same arrangement that was adopted later on petrological and geological grounds by Lossen, Rosenbusch and Walther.

CONCLUSION ON THE RISE OF PETROLOGY

Our account of the rise of petrology during the first half of the nineteenth century ends with Naumann. Early on in the second half of the century petrologists ceased for the most part to interest themselves in the possibilities as regards

the scope of their science. The threefold classification of rocks as *igneous, aqueous* and *metamorphic* by Coquand in his *Traité des roches* (1857), and its adoption by Von Cotta a few years later in the second edition of his *Gesteinslehre* (1862), more or less settled the scope of rock studies.

Since that time petrologists have settled down to a study of the microscopical, physical and chemical aspects of silicate and sedimentary rocks, with no interest in metalliferous-vein and other ore deposits so far as petrology goes. With these later developments we are not concerned in this chapter, the object of which is to show that, during the period of the rise of petrology, broader views were entertained as to the scope of this subject than have been entertained since, and that the application of the microscope to the detailed study of silicate and sedimentary rocks has led to the adoption of a narrow outlook as regards the scope of the science.

Various writers have at different times in recent years suggested broad bases of classification that would bring the study of ore deposits within the scope of petrology; but in spite of the fact that metalliferous-vein and other mineral deposits are essentially rocks in the best sense of the term, petrologists continue to ignore them. As already pointed out, however, not until all mineral deposits come within the purview of rock studies will petrology become a fully-fledged science.

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